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THE SCOPE AND LANGUAGE OF SCIENCE *

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I

I AM a physical object sitting in a physical world. Some of the forces of this physical world impinge on my surface. Light rays strike my retinas; molecules bombard my eardrums and fingertips. I strike back, emanating concentric air-waves. These waves take the form of a torrent of discourse about tables, people, molecules, light rays, retinas, air-waves, prime numbers, infinite classes, joy and sorrow, good and evil.

My ability to strike back in this elaborate way consists in my having assimilated a good part of the culture of my community, and perhaps modified and elaborated it a bit on my own account. All this training consisted in turn of an impinging of physical forces, largely other people's utterances, upon my surface, and of gradual changes in my own constitution consequent upon these physical forces. All I am or ever hope to be is due to irritations of my surface, together with such latent tendencies to response as may have been present in my original germ plasm. And all the lore of the ages is due to irritation of the surfaces of a succession of persons, together, again, with the internal initial conditions of the several individuals.

Now how is it that we know that our knowledge must depend thus solely on surface irritation and internal conditions? Only because we know in a general way what the world is like, with its

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light rays, molecules, men, retinas, and so on. It is thus our very understanding of the physical world, fragmentary though that understanding be, that enables us to see how limited the evidence is on which that understanding is predicated. It is our understanding, such as it is, of what lies beyond our surfaces, that shows our evidence for that understanding to be limited to our surfaces. But this reflection arouses certain logical misgivings: for is not our very talk of light rays, molecules, and men then only sound and fury, induced by irritation of our surfaces and signifying nothing? The world view which lent plausibility to this modest account of our knowledge is, according to this very account of our knowledge, a groundless fabrication.

To reason thus is, however, to fall into fallacy: a peculiarly philosophical fallacy, and one whereof philosophers are increasingly aware. We cannot significantly question the reality of the external world, or deny that there is evidence of external objects in the testimony of our senses; for, to do so is simply to dissociate the terms 'reality' and 'evidence' from the very applications which originally did most to invest those terms with whatever intelligibility they may have for us.

We imbibe an archaic natural philosophy with our mother's milk. In the fullness of time, what with catching up on current literature and making some supplementary observations of our own, we become clearer on things. But the process is one of growth and gradual change: we do not break with the past, nor do we attain to standards of evidence and reality different in kind from the vague standards of children and laymen. Science is not a substitute for commonsense, but an extension of it. The quest for knowledge is properly an effort simply to broaden and deepen the knowledge which the man in the street already enjoys, in moderation, in relation to the commonplace things around him. To disavow the very core of commonsense, to require evidence for that which both the physicist and the man in the street accept as platitudinous, is no laudable perfectionism; it is a pompous confusion, a failure to observe the nice distinction between the baby and the bath water.

Let us therefore accept physical reality, whether in the manner of unspoiled men in the street or with one or another degree of scientific sophistication. In so doing we constitute ourselves recipients and carriers of the evolving lore of the ages. Then, pursuing in detail our thus accepted theory of physical reality, we draw conclusions

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concerning, in particular, our own physical selves, and even concerning ourselves as lore-bearers. One of these conclusions is that this very lore which we are engaged in has been induced in us by irritation of our physical surfaces and not otherwise. Here we have a little item of lore about lore. It does not, if rightly considered, tend to controvert the lore it is about. On the contrary, our initially uncritical hypothesis of a physical world gains pragmatic support from whatever it contributes towards a coherent account of lore-bearing or other natural phenomena.

Once we have seen that in our knowledge of the external world we have nothing to go on but surface irritation, two questions obtrude themselves—a bad one and a good one. The bad one, lately dismissed, is the question whether there is really an external world after all. The good one is this: Whence the strength of our notion that there is an external world? Whence our persistence in representing discourse as somehow *about* a reality, and a reality beyond the irritation?

It is not as though the mere occurrence of speech itself were conceived somehow as *prima facie* evidence of there being a reality as subject matter. Much of what we say is recognised even by the man in the street as irreferential: 'Hello', 'Thank you', 'ho hum', these make no claims upon reality. These are physical responses on a par, semantically, with the patellar reflex. Whence then the idea of scientific objectivity? Whence the idea that language is occasionally descriptive in a way that other quiverings of irritable protoplasm are not?

This is a question for the natural science of the external world: in particular, for the psychology of human animals. The question has two not quite separable parts: whence the insistence on a world of reference, set over against language? and whence the insistence on a world of external objects, set over against oneself? Actually we can proceed to answer this two-fold question plausibly enough, in a general sort of way, without any very elaborate psychologising.

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Let us suppose that one of the early words acquired by a particular child is 'red'. How does he learn it? He is treated to utterances of the word simultaneously with red presentations; further, his own babbling is applauded when it approximates to 'red' in the presence of red. At length he acquires the art of applying the word neither

too narrowly nor too broadly for his mother's tastes. This learning process is familiar to us under many names : association, conditioning, training, habit formation, reinforcement and extinction, induction.

Whatever our colleagues in the laboratory may discover of the inner mechanism of that process, we may be sure of this much : the very possibility of it depends on a prior tendency on the child's part to weight qualitative differences unequally. Logically, as long as *a*, *b*, and *c* are three and not one, there is exactly as much difference between *a* and *b* as between *a* and *c* ; just as many classes, anyway, divide *a* from *b* (i.e. contain one and not the other) as *a* from *c*. For the child, on the other hand, some differences must count for more than others if the described process of learning 'red' is to go forward at all. Whether innately or as a result of pre-linguistic learning, the child must have more tendency to associate a red ball with a red ball than with a yellow one ; more tendency to associate a red ball with a red ribbon than with a blue one ; and more tendency to dissociate the ball from its surroundings than to dissociate its parts from one another. Otherwise no training could mould the child's usage of the word 'red', since no future occasion would be more strongly favoured by past applications of the word than any other. A working appreciation of something like 'natural kinds', a tendency anyway to respond in different degrees to different differences, has to be there before the word 'red' can be learned.

At the very beginning of one's learning of language, thus, words are learned in relation to such likenesses and contrasts as are already appreciated without benefit of words. No wonder we attribute those likenesses and contrasts to real stuff, and think of language as a superimposed apparatus for talking *about* the real.

The likenesses and contrasts which underlie one's first learning of language must not only be pre-verbally appreciable ; they must, in addition, be intersubjective. Sensitivity to redness will avail the child nothing, in learning 'red' from the mother, except in so far as the mother is in a position to appreciate that the child is confronted with something red. Hence, perhaps, our first glimmerings of an external world. The most primitive sense of externality may well be a sense of the mother's reinforcement of likenesses and contrasts in the first phases of word-learning. The real is thus felt, first and foremost, as prior to language and external to oneself. It is the stuff that mother vouches for and calls by name.

This priority of the non-linguistic to the linguistic diminishes as

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learning proceeds. *Scholarship* sets in ; i.e. the kind of learning which depends on prior learning of words. We learn 'mauve' at an advanced age, through a verbal formula of the form 'the colour of' or 'a colour midway between'. And the scholarly principle takes hold early ; the child will not have acquired many words before his vocabulary comes to figure as a major agency in its own increase. By the time the child is able to sustain rudimentary conversation in his narrow community, his knowledge of language and his knowledge of the world are a unitary mass.

Nevertheless, we are so overwhelmingly impressed by the initial phase of our education that we continue to think of language generally as a secondary or superimposed apparatus for talking about real things. We tend not to appreciate that most of the things, and most of the supposed traits of the so-called world, are learned through language and believed in by a projection from language. Some uncritical persons arrive thus at a copy theory of language : they look upon the elements of language as names of elements of reality, and true discourse as a map of reality. They project vagaries of language indiscriminately upon the world, stuffing the universe with ands and ors, singulars and plurals, definites and indefinites, facts and states of affairs, simply on the ground that there are parallel elements and distinctions on the linguistic side.

The general task which science sets itself is that of specifying how reality 'really' is : the task of delineating the structure of reality as distinct from the structure of one or another traditional language (except, of course, when the science happens to be grammar itself). The notion of reality independent of language is carried over by the scientist from his earliest impressions, but the facile reification of linguistic features is avoided or minimised.

But how is it possible for scientists to be thus critical and discriminating about their reifications ? If all discourse is mere response to surface irritation, then by what evidence may one man's projection of a world be said to be sounder than another's ? If, as suggested earlier, the terms 'reality' and 'evidence' owe their intelligibility to their applications in archaic commonsense, why may we not then brush aside the presumptions of science ?

The reason we may not is that science is itself a continuation of commonsense. The scientist is indistinguishable from the common man in his sense of evidence, except that the scientist is more careful. This increased care is not a revision of evidential standards, but only

the more patient and systematic collection and use of what anyone would deem to be evidence. If the scientist sometimes overrules something which a superstitious layman might have called evidence, this may simply be because the scientist has other and contrary evidence which, if patiently presented to the layman bit by bit, would be conceded superior. Or it may be that the layman suffers from some careless chain of reasoning of his own whereby, long since, he came wrongly to reckon certain types of connection as evidential: wrongly in that a careful survey of his own ill-observed and long-forgotten steps would suffice to disabuse him. (A likely example is the 'gambler's fallacy'—the notion that the oftener black pays the likelier red becomes.)

Not that the layman has an explicit standard of evidence—nor the scientist either. The scientist begins with the primitive sense of evidence which he possessed as layman, and uses it carefully and systematically. He still does not reduce it to rule, though he elaborates and uses sundry statistical methods in an effort to prevent it from getting out of hand in complex cases. By putting nature to the most embarrassing tests he can devise, the scientist makes the most of his lay flair for evidence; and at the same time he amplifies the flair itself, affixing an artificial proboscis of punch-cards and quadrille paper.

Our latest question was, in brief, how science gets ahead of commonsense; and the answer, in a word, is 'system'. The scientist introduces system into his quest and scrutiny of evidence. System, moreover, dictates the scientist's hypotheses themselves: those are most welcome which are seen to conduce most to simplicity in the over-all theory. Predictions, once they have been deduced from hypotheses, are subject to the discipline of evidence in turn; but the hypotheses have, at the time of hypothesis, only the considerations of systematic simplicity to recommend them. In so far, simplicity itself—in some sense of this difficult term—counts as a kind of evidence; and scientists have indeed long tended to look upon the simpler of the two hypotheses as not merely the more likable, but the more likely. Let it not be supposed, however, that we have found at last a type of evidence that is acceptable to science and foreign to commonsense. On the contrary, the favouring of the seemingly simpler hypothesis is a lay habit carried over by science. The quest of systematic simplicity seems peculiarly scientific in spirit only because science is what it issues in.

The notion of a reality independent of language is derived from earliest impressions, if the speculations in the foregoing pages are right, and is then carried over into science as a matter of course. The stress on externality is likewise carried over into science, and with a vengeance. For the sense of externality has its roots, if our speculations are right, in the intersubjectivity which is so essential to the learning of language ; and intersubjectivity is vital not only to language but equally to the further enterprise, likewise a social one, of science. All men are to qualify as witnesses to the data of science, and the truths of science are to be true no matter who pronounces them. Thus it is that science has got on rather with masses and velocities than with likes and dislikes. And thus it is that when science does confront likes and dislikes it confronts them as behaviour, intersubjectively observable. Language in general is robustly extravert, but science is more so.

It would be unwarranted rationalism to suppose that we can stake out the business of science in advance of pursuing science and arriving at a certain body of scientific theory. Thus consider, for the sake of analogy, the smaller task of staking out the business of chemistry. Having got on with chemistry, we can describe it *ex post facto* as the study of the combining of atoms in molecules. But no such clean-cut delimitation of the business of chemistry was possible until that business was already in large measure done. Now the situation is similar with science generally. To describe science as the domain of cognitive judgment avails us nothing, for the definiens here is in as urgent need of clarification as the definiendum. Taking advantage of existing scientific work, however, and not scrupling to identify ourselves with a substantive scientific position, we can then delineate the scientific objective, or the cognitive domain, to some degree. It is a commonplace predicament to be unable to formulate a task until half done with it.

Thought, if of any considerable complexity, is inseparable from language—in practice surely and in principle quite probably. Science, though it seeks traits of reality independent of language, can neither get on without language nor aspire to linguistic neutrality. To some degree, nevertheless, the scientist can enhance objectivity and diminish the interference of language, by his very choice of language. And we, concerned to bare the essence of scientific discourse, can profitably

rework the language of science beyond what might reasonably be urged upon the practising scientist. To such an operation we now turn.

In a spirit thus not of practical language reform but of philosophical schematism, we may begin by banishing what are known as *indicator words* (Goodman) or *egocentric particulars* (Russell): 'I', 'you', 'this', 'that', 'here', 'there', 'now', 'then', and the like. This we clearly must do if the truths of science are literally to be true independently of author and occasion of utterance. It is only thus, indeed, that we come to be able to speak of sentences, i.e. certain linguistic forms, as true and false. As long as the indicator words are retained, it is not the sentence but only the several events of its utterance that can be said to be true or false.

Besides indicator words, a frequent source of fluctuation in point of truth and falsity is ordinary ambiguity. One and the same sentence, *qua* linguistic form, may be true in one occurrence and false in another because the ambiguity of a word in it is differently resolved by attendant circumstances on the two occasions. The ambiguous sentence 'Your mothers bore you' is likely to be construed in one way when it follows on the heels of a sentence of the form ' x bore y ', and in another when it follows on the heels of a sentence of the form ' x bores y '.

In Indo-European languages there is also yet a third conspicuous source of fluctuation in point of truth and falsity; viz. tense. Actually tense is just a variant of the phenomenon of indicator words; the tenses can be paraphrased in terms of tenseless verbs governed by the indicator word 'now', or by 'before now', etc.

How can we avoid indicator words? We can resort to personal names or descriptions in place of 'I' and 'you', to dates or equivalent descriptions in place of 'now', and to place-names or equivalent descriptions in place of 'here'. It may indeed be protested that something tantamount to the use of indicator words is finally unavoidable, at least in the teaching of the terms which are to be made to supplant the indicator words. But this is no objection; all that matters is the *subsequent* avoidability of indicator words. All that matters is that it be possible in principle to couch science in a notation such that none of *its* sentences fluctuates between truth and falsity from utterance to utterance. Terms which are primitive or irreducible, from the point of view of that scientific notation, may still be intelligible to us only through explanations in an ordinary language rife with indicator words, tense, and ambiguity. Scientific

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language is in any event a splinter of ordinary language, not a substitute.

Granted then that we can rid science of indicator words, what would be the purpose? A kind of objectivity, to begin with, congenial to the scientific temper: truth becomes invariant with respect to speaker and occasion. At the same time a more urgent purpose is served: that of simplifying and facilitating a basic department of science, viz. deductive logic. For, consider e.g. the very elementary canons of deduction which lead from ' p and q ' to ' p ', and from ' p ' to ' p or q ', and from ' p and if p then q ' to ' q '. The letter ' p ', standing for any sentence, turns up twice in each of these rules; and clearly the rules are unsound if the sentence which we put for ' p ' is capable of being true in one of its occurrences and false in the other. But to formulate logical laws in such a way as not to depend thus upon the assumption of fixed truth and falsity would be decidedly awkward and complicated, and wholly unrewarding.

In practice certainly one does not explicitly rid one's scientific work of indicator words, tense, and ambiguity, nor does one limit one's use of logic to sentences thus purified. In practice one merely *supposes* all such points of variation fixed for the space of one's logical argument; one does not need to resort to explicit paraphrase, except at points where local shifts of context *within* the logical argument itself threaten equivocation.

This practical procedure is often rationalised by positing abstract entities, 'propositions', endowed with all the requisite precision and fixity which is wanting in the sentences themselves; and then saying that it is with propositions, and not their coarse sentential embodiments, that the laws of logic really have to do. But this posit achieves only obscurity. There is less mystery in imagining an idealised form of scientific language in which sentences are so fashioned as never to vacillate between truth and falsity. It is significant that scientific discourse actually does tend toward this ideal, in proportion to the degree of development of the science. Ambiguities and local and epochal biases diminish. Tense, in particular, gives way to a four-dimensional treatment of space-time.

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A basic form for sentences of science may then be represented as ' Fa ', where ' a ' stands in place of a singular term referring to some object, from among those which exist according to the scientific

theory in question, and ' F ' stands in place of a general term or predicate. The sentence ' Fa ' is true if and only if the object fulfils the predicate. No tense is to be read into the predication ' Fa '; any relevant dating is to be integral rather to the terms represented by ' F ' and ' a '.

Compound sentences are built up of such predications with help of familiar logical connectives and operators: 'and', 'not', the universal quantifier ' (x) ' ('each object x is such that'), and the existential quantifier ' $(\exists x)$ ' ('at least one object x is such that'). An example is ' (x) not $(Fx$ and not $Gx)$ ', which says that no object x is such that Fx and not Gx ; briefly, every F is a G .

A given singular term and a given general term or predicate will be said to *correspond* if the general term is true of just one object, viz. the object to which the singular term refers. A general term which thus corresponds to a singular term will of course be 'of singular extension', i.e. true of exactly one object; but it belongs nevertheless to the grammatical category of general terms, represented by the ' F ' rather than the ' a ' of ' Fa '. Now the whole category of singular terms can, in the interests of economy, be swept away in favour of general terms, viz. the general terms which correspond to those singular terms. For, let ' a ' represent any singular term, ' F ' any corresponding general term, and ' $\dots a \dots$ ' any sentence we may have cared to affirm containing ' a '. Then we may instead dispense with ' a ' and affirm ' $(\exists x)(Fx$ and $\dots x \dots)$ '. Clearly this will be true if and only if ' $\dots a \dots$ ' was true. If we want to go on explicitly to remark that the object fulfilling ' F ' is unique, we can easily do that too, thus:

$$(x)(y) \text{ not } [Fx \text{ and } Fy \text{ and not } (x = y)]$$

provided that the identity sign ' $=$ ' is in our vocabulary.

How, it may be asked, can we be sure there will be a general term corresponding to a given singular term? The matter can be viewed thus: we merely *re-parse* what had been singular terms as general terms of singular extension, and what had been reference-to as truth-of, and what had been ' $\dots a \dots$ ' as ' $(\exists x)(Fx$ and $\dots x \dots)$ '. If the old singular term was a proper name learned by ostension, then it is re-parsed as a general term similarly learned.

The recent reference to ' $=$ ' comes as a reminder that relative general terms, or polyadic predicates, must be allowed for along with the monadic ones; i.e. the atomic sentences of our regimented

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scientific language will comprise not only ' Fx ', ' Fy ', ' Gx ', etc., but also ' Hxy ', ' Hzx ', ' Jyz ', ' $Kxyz$ ', and the like, for appropriately interpreted predicates ' F ', ' G ', ' H ', ' J ', ' K ', etc. (whereof ' H ' might in particular be interpreted as ' $=$ '). The rest of the sentences are built from these atomic ones by 'and', 'not', ' (x) ', ' (y) ', etc. Singular terms ' a ', ' b ', etc. can, we have seen, be left out of account. So can the existential quantifiers ' $(\exists x)$ ', ' $(\exists y)$ ', etc., since ' $(\exists x)$ ' can be paraphrased 'not (x) not'.

Besides simple singular terms there are operators to reckon with, such as '+', which yield complex singular terms such as ' $x + y$ '. But it is not difficult to see how these can be got rid of in favour of corresponding polyadic predicates—e.g. a predicate ' Σ ' such that ' Σzxy ' means that z is $x + y$.

This pattern for a scientific language is evidently rather confining. There are no names of objects. Further, no sentences occur within sentences save in contexts of conjunction, negation, and quantification. Yet it suffices very generally as a medium for scientific theory. Most or all of what is likely to be wanted in a science can be fitted into this form, by dint of constructions of varying ingenuity which are familiar to logic students. To take only the most trivial and familiar example, consider the 'if-then' idiom; it can be managed by rendering 'if p then q ' as 'not $(p$ and not $q)$ '.

It may be instructive to dwell on this example for a moment. Notoriously, 'not $(p$ and not $q)$ ' is no translation of 'if p then q '; and it need not pretend to be. The point is merely that in the places where, at least in mathematics and other typical scientific work, we would ordinarily use the 'if-then' construction, we find we can get on perfectly well with the substitute form 'not $(p$ and not $q)$ ', sometimes eked out with a universal quantifier. We do not ask whether our reformed idiom constitutes a genuine semantical analysis, somehow, of the old idiom; we simply find ourselves ceasing to depend on the old idiom in our technical work. Here we see, in paradigm, the contrast between linguistic analysis and theory construction.

5

The variables ' x ', ' y ', etc., adjuncts to the notation of quantification, bring about a widening of the notion of sentence. A sentence which contains a variable without its quantifier (e.g. ' Fx ' or ' $(y)Fxy$ ', lacking ' (x) ') is not a sentence in the ordinary true-or-false sense;

it is true *for* some values of its free variables, perhaps, and false for others. Called an *open* sentence, it is akin rather to a predicate : instead of having a *truth value* (truth or falsity) it may be said to have an *extension*, this being conceived as the class of those evaluations of its free variables for which it is true. For convenience one speaks also of the extension of a closed sentence, but what is then meant is simply the truth value.

A compound sentence which contains a sentence as a component clause is called an *extensional* context of that component sentence if, whenever you supplant the component by any sentence with the same extension, the compound remains unchanged in point of its own extension. In the special case where the sentences concerned are closed sentences, then, contexts are extensional if all substitutions of truths for true components and falsehoods for false components leave true contexts true and false ones false. In the case of closed sentences, in short, extensional contexts are what are commonly known as truth functions.

It is well known, and easily seen, that the conspicuously limited means which we have lately allowed ourselves for compounding sentences—viz., ‘and’, ‘not’, and quantifiers—are capable of generating only extensional contexts. It turns out, on the other hand, that they confine us no more than that ; the *only* ways of imbedding sentences within sentences which ever obtrude themselves, and resist analysis by ‘and’, ‘not’, and quantifiers, prove to be contexts of other than extensional kind. It will be instructive to survey them.

Clearly *quotation* is, by our standards, non-extensional ; we cannot freely put truths for truths and falsehoods for falsehoods within quotation, without affecting the truth value of a broader sentence whereof the quotation forms a part. Quotation, however, is always dispensable in favour of spelling. Instead e.g. of :

Heraclitus said ‘ *πάντα ῥεῖ* ’
‘ *πάντα ῥεῖ* ’ contains three syllables,

we can say (following Tarski) :

Heraclitus said pi-alpha-nu-tau-alpha-space-rho-epsilon-iota,
and correspondingly for the other example, thus availing ourselves of names of letters together with a hyphen by way of concatenation sign. Now, whereas the quotational version showed a sentence (the Greek one) imbedded within a sentence, the version based on spelling does

not; here, therefore, the question of extensionality no longer arises.

Under either version, we are talking about a certain object—a linguistic form—with help, as usual, of a singular term which refers to that object. Quotation produces one singular term for the purpose; spelling another. Quotation is a kind of picture-writing, convenient in practice; but it is rather spelling that provides the proper analysis for purposes of the logical theory of signs.

We saw lately that singular terms are never finally needed. The singular terms involved in spelling, in particular, can of course finally be eliminated in favour of a notation of the sort envisaged in recent pages, in which there are just predicates, quantifiers, variables, 'and', and 'not'. The hyphen of concatenation then gives way to a triadic predicate analogous to the ' Σ ' of § 4, and the singular terms 'pi', 'alpha', etc. give way to general terms which 'correspond' to them in the sense of § 4.

A more seriously non-extensional context is indirect discourse: 'Heraclitus said that all is flux'. This is not, like the case of quotation, a sentence about a specific and namable linguistic form. Perhaps, contrary to the line pursued in the case of quotation, we must accept indirect discourse as involving an irreducibly non-extensional occurrence of one sentence in another. If so, then indirect discourse resists the schematism lately put forward for scientific language.

It is the more interesting, then, to reflect that indirect discourse is in any event at variance with the characteristic objectivity of science. It is a subjective idiom. Whereas quotation reports an external event of speech or writing by an objective description of the observable written shape or spoken sound, on the other hand indirect discourse reports the event in terms rather of a subjective projection of oneself into the imagined state of mind of the speaker or writer in question. Indirect discourse is quotation minus objectivity and precision. To marshall the evidence for indirect discourse is to revert to quotation.

It is significant that the latitude of paraphrase allowable in indirect discourse has never been fixed; and it is more significant that the need of fixing it is so rarely felt. To fix it would be a scientific move, and a scientifically unmotivated one in that indirect discourse tends away from the very objectivity which science seeks.

Indirect discourse, in the standard form 'says that', is the head of a family which includes also 'believes that', 'doubts that', 'is surprised that', 'wishes that', 'strives that', and the like. The subjectivity

noted in the case of 'says that' is shared by these other idioms twice over; for what these describe in terms of a subjective projection of oneself is not even the protagonist's speech behaviour, but his subjective state in turn.

Further cases of non-extensional idiom, outside the immediate family enumerated above, are 'because' and the closely related phenomenon of the contrary-to-fact conditional. Now it is an ironical but familiar fact that though the business of science is describable in unscientific language as the discovery of causes, the notion of cause itself has no firm place in science. The disappearance of causal terminology from the jargon of one branch of science and another has seemed to mark the progress in understanding of the branches concerned.

Apart from actual quotation, therefore, which we have seen how to deal with, the various familiar non-extensional idioms tend away from what best typifies the scientific spirit. Not that they should or could be generally avoided in everyday discourse, or even in science broadly so-called; but their use dwindles in proportion as the statements of science are made more explicit and objective. We begin to see how it is that the language form schematised in § 4 might well, despite its narrow limitations, suffice for science at its purest.

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Insofar as we adhere to that idealised schematism, we think of a science as comprising those truths which are expressible in terms of 'and', 'not', quantifiers, variables, and certain predicates appropriate to the science in question. In this enumeration of materials we may seem to have an approximation to a possible standard of what counts as 'purely cognitive'. But the standard, for all its seeming strictness, is still far too flexible. To specify a science, within the described mould, we still have to say what the predicates are to be, and what the domain of objects is to be over which the variables of quantification range. Not all ways of settling these details will be congenial to scientific ideals.

Looking at actual science as a going concern, we can fix in a general way on the domain of objects. Physical objects, to begin with—denizens of space-time—clearly belong. This category embraces indiscriminately what would anciently have been distinguished as substances and as modes or states of substances. A man is a four-dimensional object, extending say eighty-three years in the time dimension.

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Each spatio-temporal part of the man counts as another and smaller four-dimensional object. A president-elect is one such, say two months long. A fit of ague is another, if for ontological clarity we identify it, as we conveniently may, with its victim for the duration of the seizure.

Contrary to popular belief, such a physical ontology has a place also for states of mind. An inspiration or a hallucination can, like the fit of ague, be identified with its host for the duration. The feasibility of this artificial identification of any mental seizure, x , with the corresponding time-slice x' of its physical host, may be seen by reflecting on the following simple manoeuvre. Where P is any predicate which we might want to apply to x , let us explain P' as true of x' if and only if P is true of x . Whatever may have been looked upon as evidence, cause, or consequence of P , as applied to x , counts now for P' as applied to x' . This parallelism, taken together with the extensionality of scientific language, enables us to drop the old P and x from our theory and get on with just P' and x' , rechristened as P and x . Such, in effect, is the identification. It leaves our mentalistic idioms fairly intact, but reconciles them with a physical ontology.

This facile physicalisation of states of mind rests in no way on a theory of parallelism between nerve impulses, say, or chemical concentrations, and the recurrence of predetermined species of mental state. It might well be, now and forever, that the only way of guessing whether a man is inspired, or depressed, or deluded, or in pain, is by asking him or by observing his gross behaviour; not by examining his nervous workings, albeit with instruments of undreamed-of subtlety. Discovery of the suggested parallelism would be a splendid scientific achievement, but the physicalisation here talked of does not require it.

This physicalisation does not, indeed, suffice to make 'inspiration', 'hallucination', 'pain', and other mentalistic terms acceptable to science. Though these become concrete general terms applicable to physical objects, viz. time-slices of persons, still they may, some or others of them, remain too vague for scientific utility. Disposition terms, and other predicates which do not lend themselves to immediate verification, are by no means unallowable as such; but there are better and worse among them. When a time-slice of a person is to be classified under the head of inspiration or hallucination, and when not, may have been left too unsettled for any useful purpose. But what is

then at stake is the acceptability of certain predicates, and not the acceptability of certain objects, values of variables of quantification.

Let us not leave the latter topic quite yet: ontology, or the values available to variables. As seen, we can go far with physical objects. They are not, however, known to suffice. Certainly, as just now argued, we do not need to add mental objects. But we do need to add *abstract* objects, if we are to accommodate science as currently constituted. Certain things we want to say in science may compel us to admit into the range of values of the variables of quantification not only physical objects but also classes and relations of them; also numbers, functions, and other objects of pure mathematics. For, mathematics—not uninterpreted mathematics, but genuine set theory, logic, number theory, algebra of real and complex numbers, differential and integral calculus, and so on—is best looked upon as an integral part of science, on a par with the physics, economics, etc. in which mathematics is said to receive its applications.

Researches in the foundations of mathematics have made it clear that all of mathematics in the above sense can be got down to logic and set theory, and that the objects needed for mathematics in this sense can be got down to a single category, that of *classes*—including classes of classes, classes of classes of classes, and so on. Our tentative ontology for science, our tentative range of values for the variables of quantification, comes therefore to this: physical objects, classes of them, classes in turn of the elements of this combined domain, and so on up.

We have reached the present stage in our characterisation of the scientific framework not by reasoning *a priori* from the nature of science *qua* science, but rather by seizing upon traits of the science of our day. Special traits thus exploited include the notion of physical object, the four-dimensional concept of space-time, the classical mould of modern classical mathematics, the true-false orientation of standard logic, and indeed extensionality itself. One or another of these traits might well change as science advances. Already the notion of a physical object, as an intrinsically determinate portion of the space-time continuum, squares dubiously with modern developments in quantum mechanics. Savants there are who even suggest that the findings of quantum mechanics might best be accommodated by a revision of the true-false dichotomy itself.

To the question, finally, of admissible predicates. In general we may be sure that a predicate will lend itself to the scientific enter-

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prise only if it is relatively free from vagueness in certain crucial respects. If the predicate is one which is mainly to be used in application to the macroscopic objects of common sense, then there is obvious utility in there being a general tendency to agreement, among observers, concerning its application to those objects ; for it is in such applications that the intersubjective verifiability of the data of science resides. In the case of a predicate which is mainly applicable to scientific objects remote from observation or common sense, on the other hand, what is required is that it be free merely from such vagueness as might blur its theoretical function. But to say these things is merely to say that the predicates appropriate to science are those which expedite the purposes of intersubjective confirmation and theoretical clarity and simplicity. These same purposes govern also the ontological decision—the determination of the range of quantification ; for, clearly the present tentative ontology of physical objects and classes will be abandoned forthwith when we find an alternative which serves those purposes better.

In science all is tentative, all admits of revision—right down, as we have noted, to the law of the excluded middle. But ontology is, pending revision, more clearly in hand than what may be called *ideology*—the question of admissible predicates. We have found a tentative ontology in physical objects and classes, but the lexicon of predicates remains decidedly open. That the ontology should be relatively definite, pending revision, is required by the mere presence of quantifiers in the language of science ; for, quantifiers may be said to have been interpreted and understood only insofar as we have settled the range of their variables. And that the fund of predicates should be forever subject to supplementation is implicit in a theorem of mathematics ; for it is known that for any theory, however rich, there are classes which are not the extensions (cf. § 5) of any of its sentences.

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ON THE OBJECTIVE OF EINSTEIN'S WORK *

W. H. McCREA

THIS paper is an attempt to state Einstein's own purposes in his work and the significance which he himself assigned to its main stages. Throughout his scientific life, Einstein worked towards a single objective with a probably unique degree of concentration. To appreciate this and the manner in which he regarded his successive achievements as bringing him nearer to his objective is both interesting in itself and helpful in understanding the work and the fashion of its presentation.

In making this attempt, I rely upon Einstein's 'Autobiographical Notes' ¹, upon the light they shed on the work, and upon the internal evidence of the work itself. With regard to the 'Notes', I realise that even to Einstein the direction of the path he had followed must have been clearer in retrospect than it had been when he was travelling along it. Nevertheless, his review accords well with all that he had written on the way.

I do not imply that the significance assigned by Einstein is the same as others see in his work, nor that his objective was the most profitable one he could have chosen.

I *Physics before Einstein*

We are dealing throughout only with *fundamental* physics. The situation as Einstein seems to have seen it when he started work about 1900 was this :

So far as classical mechanics was concerned its only success was in the kinetic theory of matter where only the mechanics of point masses was required. Its treatment of gravitation could scarcely be deemed successful from a fundamental standpoint on account of objections to

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¹ *Albert Einstein : Philosopher-Scientist*, ed. P. A. Schlipp, New York, 1951. The 'Notes' are the first item in this collection of essays and are referred to as 'A' followed by the page number.

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the concept of action at a distance. And even though it was successful in relating various experimental properties of matter, the theory had internal difficulties such as were stressed particularly by Mach in regard to the origin of inertia.

On the other hand, the Faraday-Maxwell theory of the electromagnetic field had been successful in all its applications. Moreover, the concept of the *field* by its very nature meant that the theory did not require action at a distance. Originally, matter had been regarded as the carrier of the field, with empty space as some sort of limiting case. But a more satisfactory formulation had been given by Lorentz according to which the field exists *only* in the empty space around the particles of matter. These particles carry the charges that are the source of the field and are acted upon by the field. All attempts to construct a mechanical theory of the field through the concept of an aether had proved unsatisfactory.

About 1900 the situation was further complicated by Planck's ideas about energy-quanta, but this will be dealt with below.

Thus Einstein saw contemporary physics as depending upon two unrelated concepts, that of the *discrete-particle* and that of the *field*. The mass-particle was treated by the laws of mechanics that had proved incapable of treating the field. The particle was isolated in some setting about which the concept gave no information. On the other hand, the field was everywhere except at the particles. The field-concept admitted a view of the particle as a singularity in the field : but beyond that, as the concept existed, it provided no means of treating the particle.

2 *Einstein's objective*

Finding himself in the situation I have tried to describe, Einstein wanted to construct for himself a more unified system of physics. Clearly he regarded the field-theory part of existing physics as the more satisfactory in itself and also the more capable of extension. Were a synthesis to be attained, it was much more likely to come by elaborating the field-concept than by working from the particle-concept and the other concepts that had come to be associated with it.

Einstein in fact set himself the objective of establishing a field-theory for the whole of fundamental physics. The field-variables or functions of them were to describe the whole of physical reality : the laws of physics were to be expressed entirely as field-equations.

These field-equations were to be differential equations in which the continuous independent variables are co-ordinates of space-time (about which more will be said later). The field-equations were to be satisfied *everywhere*, this being by definition what was meant by a complete field-theory. Therefore the field-variables had (at least) to exist and be continuous everywhere. In particular, the existence of matter in any special form was to be a consequence of the field-equations. Thus a material particle would appear as some sort of 'concentration' of the field, but *not* as a singularity, i.e. a point or region at which the field-equations failed to be satisfied.

Probably Einstein's formulation of this objective took shape only by degrees, though in a general way he seems to have had it in mind from very early in his work. Also he obviously expected to progress towards it only by stages each of which would have itself to suggest the next.

Non-linearity of field-equations. In particular Einstein clearly expected that up to some stage it would suffice to treat a material particle as a point-singularity in the field. If this were done, he saw that the field-equations could not be linear. This can be understood as follows. In classical electromagnetic theory we may treat a point-charge as a singularity of a certain sort. There certainly exist solutions of Maxwell's field-equations that possess only one such singularity and that satisfy the field-equations everywhere else. Any such solution we call the field of a single point-charge moving in some known manner. Now, because these field-equations are linear, we can superpose any two such solutions. So far as the field-equations are concerned, the result is a complete solution of the problem of two point-charges. But no account is thereby taken of what we call the action of the field of either charge upon the other. In order to take this into account, the solution would have to be made to satisfy also the *equations of motion*. In classical electrodynamics, these are something that have to be postulated over and above the field-equations. Thus, in fact, classical electrodynamics is not a pure field-theory. In this way we see that a pure field-theory cannot be constructed with the use of only linear field-equations.

However, suppose the field-equations had been non-linear. Then the superposition of solutions does not in general give a fresh solution. So the freedom in finding solutions with two singularities is less than it was before. This loss of freedom would have to be interpreted as an interaction of the particles (through their fields). With suitable non-

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linear equations it could be expected to reproduce the same sort of results as did the equations of motion in the previous case. So these equations would be no longer necessary. Thus it appears that the limitation we have noticed in the case of a linear theory is not necessarily inherent in field-theories in general.

As set out, this argument applies to the treatment of particles as singularities in the field. This treatment would be regarded as an approximation from the standpoint of a complete field-theory. But it is inconceivable that the removal of this approximation would restore linearity to the field-equations. Their non-linearity must be expected as an essential feature.

3 *First phase*

In 1905 Einstein published his first three great papers on Brownian movement, on quantum theory and on relativity. As individual contributions to science their importance on account of their immediate conclusions was enormous. Also from this point of view, they are quite unrelated. However, we are here concerned with their significance from the point of view of the development of Einstein's ideas on fundamental physics in general, and in this respect they are by no means unrelated.

Brownian movement. In this work Einstein showed how the atomicity of matter could be verified by observable phenomena and how molecular dimensions could be determined. As we know, his predictions agree with experiment. It was as though Einstein asserted that it is no use trying to find a fundamental treatment of matter in terms of its atomic structure until we have established that matter is actually composed of atoms (and until we have knowledge of the sizes and masses of these atoms).

Quantum theory. Einstein's work on quantum theory was in the first place a clarification of Planck's. He established its significance as showing that energy is emitted and absorbed by matter and transferred only in discrete quanta. In doing this he was aided by the use of conceptual experiments treated as in his work on Brownian movement and by the agreement of his conclusions with actual experimental results on the photo-electric effect.

As regards matter, the outcome as he saw it was this. Matter does consist only of atoms (as shown in particular by his own work on Brownian movement): the fundamental physics of matter must be

the physics of these atoms. But these atoms behave in no wise like classical point-masses (since the atoms or combinations of atoms lose or gain energy only in quanta) : consequently, the prospect of constructing a fundamental physics of matter upon anything like the classical mechanics of point-masses is finally destroyed. The admitted success of the kinetic theory of matter shows only that classical mechanics is in some sense a valid limit of some very different treatment.

As regards the electromagnetic field, Einstein recognised the quantal behaviour as showing a failure of Maxwell's theory. But this failure appeared less radical than that of mechanics. For the classical treatment of the energy of the electromagnetic field was not a deduction from Maxwell's theory, but merely something shown to be compatible with it. The existence of energy-quanta was immediately connected with this treatment and not immediately with the theory itself. While realising the need for a better field-theory, Einstein was therefore confirmed in his view that the hope of progress lay in finding such a theory and not in any concepts of the sort associated with classical mechanics.

At this stage also Einstein gave the first statement of the double nature of radiation (corpuscular and undulatory) which in its extension also to matter by other physicists was later to play such a central part in the development of quantum mechanics. But then and ever after Einstein himself regarded this as a heuristic description only and one that called for replacement by something more in keeping with the kind of theory he aimed to develop.

Special relativity. It must be remembered that the famous paper in which Einstein founded the theory of special relativity was one 'On the Electrodynamics of Moving Bodies'. Its immediate object was to overcome well-known difficulties in the subject so described. Its astonishing success in achieving this object was significant for our study in the following ways ;

(a) It resolved the difficulties, not by modifying Maxwell's field-equations, but by modifying classical kinematics and the equations of motion. This might again be claimed as supporting Einstein's view of existing field-theory as the better established part of physics.

(b) It abolished the possibility of instantaneous action at a distance for the very good reason that it abolished the meaning of absolute simultaneity of distant events. Action at a distance propagated with the speed of light remained a possibility, though, in Einstein's view, an unnatural one on account of the difficulties to which it would lead in

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formulating conservation laws. In the absence of action at a distance, physical reality must be described by functions continuous in space and the material particle ceases to be a basic concept (A61).

(c) It effected the first significant step in the unification of physics. It did so by making the laws of dynamics and of electrodynamics invariant under the same set of transformations.

(d) It made the classical aether superfluous. This is a direct consequence of (c). For, basically, the aether had been invented to carry the electromagnetic field because the latter had been thought not to satisfy the same relativity principle as mechanics.

(e) It removed the possibility of regarding *mass* as an independent concept because the mass of an isolated system became equivalent to its energy and this is only a component of a 4-vector (A61).

Restrictions of special relativity. Special relativity is restricted to descriptions relative to any one of the set of *inertial* reference-frames. In the first place, the theory did not itself indicate what it is that makes a frame an inertial frame : in this respect the situation was exactly the same as that in classical physics and liable to the criticisms of Mach. In the second place, the theory gave no indication as to how it could be extended to descriptions relative to other reference-frames.

The reason for the latter difficulty is a simple physical one but is not generally appreciated. In classical mechanics and in special relativity a frame of reference is an actual rigid body.¹ But in classical mechanics the configuration of a rigid body is independent of its state of motion and so there is no difficulty about accelerated frames of reference. This is because in a classically ideally rigid body all effects are transmitted instantaneously. In a relativistically rigid body on the other hand, effects are transmitted with finite speeds of propagation and the configuration of the body depends upon its state of motion. Not only that, but if the body be accelerated relative to an inertial frame, there must be forces acting on it and its configuration depends also upon the points of application of the forces. In fact an accelerated rigid body simply fails to furnish a reference-frame. Special relativity cannot be extended to accelerated frames because accelerated frames of reference cannot exist in its domain of application.

It is usually thought that Einstein developed general relativity more

¹ Einstein in his first paper stated : 'The theory to be developed is based . . . on the kinematics of the rigid body. . . .' What we are now about to notice and what Einstein had, of course, later appreciated is that it involves also the *dynamics* of the rigid body.

or less directly from the principle of equivalence. But he tells us that it was seven years between his realising the significance of that principle and his seeing how to formulate its consequences (A67). The delay was on account of the difficulty I have just explained, though Einstein seems to have thought of it in slightly different terms.

Finally, special relativity did not admit a satisfactory treatment of gravitation. The reason for this is again fundamentally that a frame of reference is an actual body. The inertial frames in special relativity are actual bodies moving freely and are not relatively accelerated. Consequently it is implicitly assumed that there is no gravitational field. Special relativity is inherently incapable of dealing with a field of force that affects all bodies in the field.

The more one contemplates the restrictions of special relativity the more one marvels at the fact that classical mechanics proved to be an entirely self-consistent system. But realising the features that make it self-consistent (in particular the assumptions about rigid bodies) the more one marvels at anyone finding another system that is also self-consistent. But actually the self-consistency of general relativity is much less remarkable than that of classical mechanics and gravitation. For it is guaranteed from the outset merely by the mathematical existence of Riemannian geometry. Classical mechanics, on the other hand, is built upon a number of independently formulated postulates and there is nothing in the formulation to ensure mutual consistency.

One is also driven to wonder at the fairly ready acceptance accorded to special relativity in its early days. But there is now ample observational support of such a sort as to show that there can be no going back from it. Nevertheless from a fundamental standpoint it raises more questions than it solves and it is very clear why Einstein considered it only a beginning.

4 *Second phase*

So far as it went, special relativity had given a successful treatment of electrodynamics: it was his effort to represent a gravitational field in the theory that made Einstein see its limitations (A63). The pursuit of the problems thus presented led him far afield. Up to this stage, I think gravitation had not been mentioned in any of his work and it must have been a surprise to Einstein to find that so much of his subsequent work was concerned with it. In fact it dominated the whole of the second phase of his work which is generally considered to comprehend his greatest achievements.

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General relativity. The following seem to be the salient features for our consideration ;

(a) The theory does incorporate the *principle of equivalence* of gravitational and inertial mass. In this regard it is a notable advance upon Newtonian theory in which the equivalence has to be regarded as a not inevitable empirical result.

However, the theory does not rest directly upon the principle which plays a suggestive rather than determinative part in its formulation.

Were the theory to depend more directly upon the principle there would not be much hope for its extension to non-gravitational fields. For there is nothing to correspond to the principle of equivalence for electromagnetic fields.¹

(b) Space-time is treated as a four-dimensional continuum of events, i.e. an event is treated as an ordered set (x_1, x_2, x_3, x_4) of four real numbers and there is an event corresponding to every set of values when x_1, x_2, x_3, x_4 vary continuously and independently in some interval. The co-ordinates x_1, x_2, x_3, x_4 initially serve merely to label events and have no immediate metrical significance.

This is the fundamental break-away from classical theory and special relativity. It is necessitated by the difficulty of extending the use of rigid frames of reference beyond the scope of the special theory. Moreover, no assumption about anything like a rigid frame ought to be put in at the outset of a general theory. For this would be to make it depend upon just the sort of thing we are trying to get away from in fundamental physics.

Einstein said in effect : Let us begin with a system specified in the most rudimentary manner possible : naturally, this does not mean that the specification has no physical significance, but this significance has got to be supplied by the theory and not imposed upon it.

Now if the (continuously variable) co-ordinates are to begin with merely a way of naming events, if we make any continuous transformation of these co-ordinates, we shall have another equally good way of naming them. So if the laws of physics are to be expressed with the use of these co-ordinates, their expression must give no preference to any one set over any other. This is Einstein's *principle of covariance* which is also the same, I think, as he meant by the principle of general relativity (A69).

In this view the principle of covariance is much more than the

¹ I owe this remark to a conversation with Mr J. Moffat.

principle that the laws must be valid for all observers. It includes this, but it is primarily a recognition of the new status assigned to co-ordinates in general. Initially, there is no association of any particular co-ordinate systems with any particular observer.

It can, of course, be held that the principle of covariance is unnecessarily general. But this is to criticise the whole approach. That approach is, indeed, not compulsory, but it is hard to see what other Einstein could have taken; having taken it, the principle of covariance seems unavoidable. But I should add that I am not here giving a mathematically rigorous statement which would raise issues other than those I mention and other than those Einstein contemplated. Some of these have quite recently been formulated by Lichnérowicz.¹

(c) The next step was to introduce field-variables. The simplest way to do this in agreement with the required covariance was to take them to be tensors. Guided by the geometrical expression of special relativity, Einstein took the field to be specified by associating a single symmetric tensor of the second order g_{pq} with every event (the components being differentiable functions of the co-ordinates). This tensor turns out both to determine the metrical properties of space-time and to serve as the (tensor) potential of the gravitational field. For reasons of a rather formal character, Einstein chose as the field-equations for empty space

$$G_{pq} = 0 \quad (p, q = 1, 2, 3, 4) \quad (1)$$

where G_{pq} is a tensor derived from g_{pq} , linear in the second derivatives but quadratic in the first derivatives of the components of g_{pq} , and containing the g_{pq} themselves in a rather complicated manner. However, in a well-defined sense, this is the simplest possible law for such a field. The reasons for interpreting the field as purely gravitational are also well known.

What is most significant at this stage is that the metrical and gravitational properties are thus inseparably connected. That this must be so was shown by our considerations of frames of reference. As a qualitative feature of general relativity, this is something that can never be surrendered. It is the basic feature, and the great triumph of general relativity is its recognition and a mathematical expression of it.

(d) The equations (1) are non-linear in the field-variables. As we saw, this is a desired property. Moreover, it does produce exactly the desired consequences: Einstein (with his collaborators, 1938-49)

¹ A. Lichnérowicz, *Théories relativistes* . . ., Paris, 1955

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showed that equations (1) determine the behaviour of particles (as singularities) without the need to postulate any equations of motion.

(e) Up to this point, the theory is a wholly satisfactory theory of the pure gravitational field, according to all Einstein's criteria. But now, how is matter to be represented? Equations (1) are not sufficiently general to represent matter as well as gravitation.

Einstein chose provisionally to represent matter by replacing (1) by

$$G_{pq} = -kT_{pq} \quad (2)$$

where T_{pq} is the momentum-stress-energy tensor of the matter present, k is a universal constant, and we are interpreting G_{pq} as the Einstein tensor (not the Ricci tensor). In empty space $T_{pq} = 0$: so (2) is consistent with (1).

There are two views possible about equations (2). (i) Given any g_{pq} -field and calculating the G_{pq} , equations (2) merely state that the g_{pq} describe a system composed of matter with the stress-energy-momentum tensor G_{pq} (in suitable units). In this view, the field-equations $G_{pq} = 0$ have no profound significance: they merely specify the parts of the system that we say are empty of matter. Since there is very little restriction upon the g_{pq} -fields that we may start with, the theory tells us very little about what properties of matter are possible in physics. (If we follow up this view, we do not expect general relativity to do this.) On the other hand, we achieve a remarkable synthesis in physics because matter becomes a feature of space-time: instead of having to deal with space-time and matter as separate entities, they are now a single entity.

(ii) We regard (2) as a retreat from the pure field-theory treatment. It represents the admission of sources for the field, being on the same footing as Poisson's equation $\Delta^2 V = -4\pi\gamma\rho$ in classical potential theory. Even more simply, it is an admission that the field-equations do not hold good everywhere and that, at this stage, we have not been clever enough to find ones that do. From this point of view, the fact that the theory does not tell us what properties matter must have is a failure of the theory.

This second view was that consistently maintained by Einstein. He described the introduction of T_{pq} as a 'makeshift' device.

5 Third phase

Einstein's dominating effort in all his remaining work was to find field-equations for the total field. This was what he meant by a

unified field-theory. In keeping with the view (i) of general relativity described above, we might seek to generalise the g_{pq} -field in such a way that it provides further quantities corresponding to the G_{pq} that we could define to be the charge current-vector of the electromagnetic field. If successful, we could call the result a unified theory of gravitation and electromagnetism. But this is *not* what Einstein aimed at achieving.

Let us make the position as clear as possible. In the equation $G_{pq} = -kT_{pq}$ the left hand side is provided by the field and the right-hand side is just a new way of naming this feature of the field. The equation tells us nothing about the field. However, if we regard the equation $G_{pq} = 0$ as basic for the gravitational field, we interpret the terms on the left as certain features of the field (G_{pq} stands for several terms in g_{pq} and its derivatives) and the equation tells us that these features are related in a certain way. Einstein wanted corresponding equations for the total field, so that there would be terms on the left that could be interpreted as representing every possible feature of the field. The left-hand side would give us everything that could exist and the equations would tell us how these things are related. This was the sort of theory Einstein sought. I think it is true to say that we know no example of such a theory.

Einstein considered that the only way to get such a theory was in the same general way as that in which he had got his gravitational theory of empty space (A89). This meant primarily that the theory would not be suggested by experimental facts, but by the search for a suitable mathematical structure. So his work consisted in trying to find a structure of a suitable degree of complexity. From 1928 onwards he published several attempts that he later discarded, until in 1948 and later developments he thought he had the correct approach in working with a non-symmetrical tensor g_{pq} . He proposed a set of field-equations for the total field. But it is not yet known whether they possess singularity-free solutions, still less whether such solutions, if they exist, are in agreement with experience.

6 Comments

I have not mentioned the several important 'orthodox' advances made by Einstein in both relativity theory and quantum theory. Nor have I said anything about his work on cosmology. Also I have not discussed his long controversy with the upholders of the standard

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interpretation of modern quantum theory. This interpretation denies the possibility of what Einstein called 'an exhaustive description of physical reality on the basis of the continuum' (A93) and was what he aimed at achieving.

Even though almost all physicists believe that a fundamental description of physical reality is not possible in the way that Einstein tried to get it, it is important to try to understand his position as expressed by his doctrine of our freedom in constructing the concepts that we use in ordering our thoughts about our empirical experience.

Finally a word about field theories in general, of the sort contemplated by Einstein. We could say that any such theory would consist of a 'geometry' together with a set of field-equations. That is to say, we select a geometry of a certain generality, then we proceed to restrict that generality by requiring the geometry to satisfy the field-equations and finally we assert that the geometry so restricted is a representation of physical reality. Now, on the one hand, we could have started with a yet more general geometry, so that we should require *more* field equations in order to produce the restriction to the same ultimate form as before. On the other hand, we might have started by selecting just that restricted form, in which case we should require *no* field-equations. In order to represent physical reality in the latter case, we should have only to attach physical names to features of the geometry.

In the first place, this latter case would be illustrated by the above interpretation (i) of Einstein's relations (2). Thus any objection in principle to this interpretation as opposed to the interpretation (ii) seems to be illusory.

In the second place and more generally, such considerations cast doubt upon the possibility of field-equations being of any very deep significance for our understanding of physics. A mathematical description of physical reality might be much more conveniently expressed by subjecting a comparatively simple general geometry to a set of field-equations, rather than directly in terms of the less general but more complicated geometry that results. But that seems to exhaust the importance of the field-equations.

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HOBBS AND HULL—METAPHYSICIANS OF BEHAVIOUR *

R. S. PETERS AND H. TAJFEL

I *The Idea of a Universal System of Behaviour*

It is sometimes instructive to compare modern systems of thought with those of the past not simply for the sake of pointing out what startling similarities can be found, but also because the past systems are usually less cluttered up with details and it is easier to see the logical difficulties they involve. This is particularly the case with mechanical systems for explaining human behaviour; for in such systems there are certain crucial logical difficulties which can too easily be covered up by the intricacy and subtle devices of the latest machine.

There are many candidates to the title of 'the father of modern psychology'. But the claims of Thomas Hobbes can be pressed very strongly in that he was not only the first to suggest that human beings are machines, but also the first to attempt a systematic explanation of *all* human actions in terms of the same principles as were used to explain the behaviour of inanimate bodies. Descartes and others thought that animal behaviour and the *involuntary* actions of men could be mechanically explained, but not distinctly human actions, involving reason and will. Hobbes ruthlessly extended Galileo's assumptions into the innermost sanctuaries of human thought and decision. He claimed originality for his civil philosophy on this account. Indeed, he hoped that his name would be as famous in the history of psychology and social science as that of Harvey who extended the new science of motion to physiology.

Hobbes sketched a Grand Plan for the explanation of human behaviour—starting with simple motions in geometry and proceeding via mechanics, physics, and physiology to psychology and social science. A more limited version of this deductive dream is to be found in recent times in the work of C. L. Hull. The title of Hull's latest book is *A Behavior System*.¹ The aims of the enterprise are explicitly stated

* Received 27 ix 55

¹ C. L. Hull, *A Behavior System*, New Haven, 1952

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both in the latest book and in its predecessor, his *Principles of Behavior*,¹ published some ten years earlier. Thus, 'the objective of the present work is the elaboration of the basic molar behavioral laws underlying the "social sciences"'.² Elsewhere, it is said that :

An ideally adequate theory even of so-called purposive behavior ought, therefore, to begin with colorless movement and mere receptor impulses as such, and from these build up step by step both adaptive behavior and maladaptive behavior. The present approach does not deny the molar reality of purposive acts (as opposed to movement), of intelligence, of insight, of goals, of intents, of strivings, or of value ; on the contrary, we insist upon the genuineness of these forms of behavior. We hope ultimately to show the logical right to the use of such concepts by deducing them as secondary principles from more elementary objective primary principles.³

In the concluding pages of the *Principles of Behavior*, the Grand Plan is given an even more ambitious and more detailed expression. Through a 'systematization of the behavior sciences' based on the consistent use of certain methodological rules, Hull hopes that ultimately treatises 'on the different aspects of the behavior sciences will appear'. These treatises will be based on systematic primary principles, and will present general or specific theories of individual and social behaviour, of 'communicational symbolism or language', of 'social or ritualistic symbolism', of economic, moral, and aesthetic valuation,

of familial behavior ; of individual adaptive efficiency (intelligence) ; of the formal educative processes ; of psychogenic disorders ; of social control and delinquency ; of character and personality ; of culture and acculturation ; of magic and religious practices ; of custom, law and jurisprudence ; of politics and government ; and of many other specialised fields of behavior.⁴

Now it would be very welcome to have a deductive system in which statements about human behaviour could be deduced from more general laws—e.g. of mechanics or physiology. But it may well be that this programme is a pipe-dream—especially if the model is based on mechanics. For the difficulties in developing such a system may not be empirical ones connected with the complexity of human behaviour, as is often thought, but *logical* ones connected with the categories of description appropriate to human action.

¹ C. L. Hull, *Principles of Behavior*, New York, 1943

² Hull, op. cit. p. 17

³ *ibid.*, p. 25-26

⁴ *ibid.*

It used to be held that man was a rational animal and that his reason was of a different ontological status from the rest of his body—not subject to the laws of nature. As often, this metaphysical thesis may well have enshrined an important logical truth, namely that man is a rule-following animal and that adequate explanations in terms of efficient causes *alone* cannot be given for actions which are in accordance with rules, conventions, criteria, canons, and so on. The old time-honoured gulf between nature and convention may well have far more general application than is often realised.

It is our thesis that there are certain logical difficulties about *any* mechanical system of human behaviour. These exhibit themselves in a deductive system as gulfs

(a) between motions at a physiological level and human actions which are goal directed and usually conform to certain criteria or conventions,

(b) between motions of the body and consciousness—especially rational thought.

These gaps may well all be connected with man's peculiarity as a rule-following animal. Our hope in this paper is to exhibit the rather surprising similarity between the systems of Hobbes and Hull, and to substantiate, in places where the similarity between the systems is most apparent, the general thesis that mechanical explanations can never be *sufficient* for actions falling under the concept of rule-following.

2 *Motions and Human Actions:*

The Similarity between the Theories of Hobbes and Hull

The basic presupposition of mechanistic explanation is that all causes are antecedent motions. As Hobbes put it, there can be no action at a distance, 'no cause of motion, except in a body contiguous and moved'.¹ Now a great many things happen for which there is presumably some cause, yet it is difficult to see any motion in a contiguous body which could have caused it. Recourse is therefore made to the notion of unobservable motions either within or between bodies. Hobbes exploited this move with considerable ingenuity. He managed to bridge the gap between the movements in external bodies, which were transmitted by means of a medium to the sense-organs,

¹ T. Hobbes, *E.W.*, Vol. I, p. 124 (*E.W.* stands for *English Works* and is the standard way of referring to the Molesworth edition of Hobbes' Works. Similarly *L.W.* stands for *Latin Works*)

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and the movements of the body in appetite and aversion by introducing the concept of 'endeavour' or 'conatus', which he defined as 'motion made in less space and time than can be given; that is motion made through the length of a point and in an instant or point of time'.¹ It was a term for *infinitely small* motions which he took over from the physical scientists and generalised to bridge the gap between physics, physiology, and psychology. It was a peculiarly subtle move; for although the term was used as a physical construct at the molecular level, it conveyed the suggestion of striving and direction which was so apt for the transition to psychological happenings at the molar level. So wherever there was a gap in observable motion—e.g. between the object and the sense-organ or between the stimulation of the sense-organ and the movements of the muscles in appetite and aversion, Hobbes postulated 'endeavours' which transmitted the motion.² For, according to his theory, motions from the external world not only move to the brain and produce images; they also affect the vital motions of the body which are manifest in the circulation of the blood, breathing, excretion, nutrition, and other such processes. When these incoming motions impede the vital motions, this is felt as pain and the parts of the body are acted on by the muscles 'which is done when the spirits are carried now into these, now into other nerves, till the pain, as far as possible, be quite taken away'.³ Similarly in the case of pleasure, the spirits are guided by the help of the nerves to preserve and augment the motion. When this build-up of endeavours tends towards things known by experience to be pleasant, it is called an appetite; when it tends to the avoidance of what is painful, it is called an aversion. Appetite and aversion are thus 'the first endeavours of animal motion'. Even in the case of a few appetites and aversions which are born with men, such as those for food, excretion, etc., (which sound very much like the modern 'primary drives'), initiation of movement is from without.

Hull's system is surprisingly similar; he starts, as Hobbes did, from the simplest possible elements. An adequate theory of behaviour, he

¹ T. Hobbes, *E.W.*, Vol. I, p. 206

² The concept of 'endeavour' also enabled Hobbes to give a substantial interpretation of dispositional terms. On his view, when we ascribe a 'power' or capacity to anything, we are making a statement about an actual build-up of minute motions. Even habits were explained as *actual motions* made more easy and more ready by perpetual endeavours.

³ T. Hobbes, *E.W.*, Vol. I, p. 407

says, ought 'to begin with colorless movements and mere receptor impulses as such, and from these build up step by step both adaptive and maladaptive behavior'.¹ For Hobbes action was an outcome of an interplay between internal and external motions. Hull's analysis of the initiation of action is also based on an interplay of assumed minute motions within the 'neural structures'. Observable actions of the organism are for him, in most cases, the result of existing 'habit structures' slowly built up on the basis of previous experience, according to certain principles specified in his postulates. There is no direct cause-and-effect sequence, as in Hobbes, between the properties of the present stimulation and the consequent actions. But Hull's picture, made much more complex by the intervention of the past through learning, remains nevertheless an essentially mechanical picture. The extrapolation from minute occurrences to behaviour, while not based on a direct link between sensation and action, or external and internal motions following each other in a simple manner, is based on 'habit structures' built into the nervous system during the past, and active at the time of stimulation. The main difference here between Hobbes and Hull is not a difference of principle: it consists in the fact that Hull specifies the conditions of the past motions (learning) which led to the pattern of motions as it is observed in the present. The passing of the organism into action is the result of the preponderance of the 'strongest' of these motions. The concepts used by Hull at this stage of his analysis are stated in mechanical terms. A threshold is 'a quantum of resistance or inertia which must be overcome by an opposing force before the latter can pass over into action'.² The 'competition of reaction potentials' is basically a conflict of 'motions', the strongest of which 'wins', and thus determines action. The 'behavioral oscillation', a concept introduced in order to account theoretically for those unpredictable movements of the organism which could not be entirely explained by the momentary status quo between the competing 'reaction potentials', is conceived as an outcome of an infinite number of minute motions.

The basic principles concerning the inner workings of motives and incentives are very similar in both systems. Hobbes is concerned with a mechanical explanation of pain and pleasure. Hull is in need of simple assumptions, which would allow him to describe the 'mechanism' by which successful (i.e. rewarded) responses remain a part of

¹ T. Hobbes, *E.W.*, Vol. I, p. 25

² *ibid.* p. 323

the organism's habit equipment, while the unsuccessful ones are eliminated. Hobbes assumes an increase and decrease in vital motions. Hull's reductionism goes one step further. In his simplified scheme the nature of reinforcement consists essentially in a reduction in the internal stimulation (e.g. in hunger, thirst, fear) which follows the successful response. The locus of this reduction must, by necessity, remain vague. It is applied to primary drives by assuming, in each case, some specific internal pattern of stimulation to be reduced. More complex forms of motivation are reducible to the basic mechanism by a transition in which both the incentive nature of previously rewarded situations, and the intervention of some kind of stimulation to be reduced (e.g. anxiety) play their part.

The 'drive-reduction hypothesis' is the equivalent of Hobbes' decrease in vital motions. But Hobbes was content with the statement of the general principle, which then allowed him to go on talking about motivation in terms of efficient mechanical causes. Hull attempts to be more specific: the 'minute unobservable' finds its way into an explanation of 'secondary motivation'. The most explicit attempt at generalising the principle to various forms of human endeavour can be found in a recent paper by Brown,¹ in which anxiety reduction is made the basis of assigning to the 'reduction principle' the capacity of explaining a very wide range of human motivational phenomena.

As a matter of fact Hobbes did something rather similar in his theory of the passions, though at the molar level and without any pretence of relating his theory of 'passions' to his physiological theory; for all the 'passions' are represented as manifestations either of the desire for power or of the fear of death. Laughter, for instance, is explained as an expression of sudden glory when we light upon some respect in which we are superior to others; courage is aversion with hope of avoiding hurt by resistance; and pity is grief for the calamity of another rising from the imagination that a like calamity may befall ourselves. The reduction of all passions to the desire for power and the fear of death provided Hobbes with an exciting psychological analysis of politics and with great opportunities for coining epigrams; but it had a tenuous connection only with the physiological details of his theory of motivation. The Hullian reduction of complex behaviour,

¹ J. S. Brown, 'Problems Presented by the Concept of Acquired Drives', *Current Theory and Research in Motivation: A Symposium*, 1953

on the other hand, sketches a simplified 'picture' of our internal workings and transfers physiological description to behaviour at the molar level. And the use of 'avoidance behaviour' (such as behaviour due to anxiety) to redescribe other forms of motivation in terms of its negative forms is due, to a large extent, to the fact that 'avoidance behaviour' can be quite easily described in terms of reduction of internal stimulation. It can thus be linked with a vague physiological 'picture'; but, apart from this dubious advantage, its merits as an explanation are very questionable.¹

3 *The Illegitimacy of the Transition from Motions to Human Actions*

The link with physiology, which we have described as 'a dubious advantage' is regarded by Hull as the chief strength of his theory. For he claims that eventually descriptions of actions at the molar level will be deducible from physiological postulates at the molecular level. But surely the link cannot be that of *deducibility*. Hamlyn² has recently discussed the confusion existing in some psychological theories, in which activities have been described in terms of movements, observable or unobservable. The distinctive features of activity, or behaviour, will be left out in such a description. For no fixed criterion can be laid down which will enable us to decide what series of movements constitutes a piece of behaviour—e.g. getting a treaty signed or winning a girl's affection. Descriptions of behaviour imply standards, which are loosely defined and which are interpretations at quite a different level from descriptions of movements. Of course behaviour involves movements; but it cannot be described simply in terms of movements. For similar pieces of behaviour can involve quite different movements.³ Some movements in the body and brain, for instance, are necessary conditions for passing an examination, but it has yet to be shown that any *particular* movements are either necessary or sufficient. Now if behaviour cannot ever be *described* purely in terms of movements, how much less can it be *deduced* from a theory which is concerned only with 'colourless movement'.

By his analysis of motivation Hobbes hoped to substantiate his

¹ See, for instance, Harlow's comments on Brown's paper: *Ibid.*, pp. 22-23.

² D. W. Hamlyn, 'Behaviour', *Philosophy*, 1953, 28, 132-145

³ A similar distinction between behaviour and physical movements was drawn in a different context by J. O. Wisdom, 'Mentality in Machines', *Proc. Arist. Soc.*, Sup. Vol. 26, 1952, 10-15.

claim that : ' A final cause has no place but in such things as have sense and will ; and this also I shall prove hereafter to be an efficient cause.' ¹ And, of course, he was right in saying that human actions have efficient causes—external stimuli, movements of the sense-organs, internal motions, and so on. But this does not mean that a list of any such movements could ever be *sufficient* to explain actions. For actions are distinguished by the goals towards which movements are directed ; the goal makes the movements part of an action of a certain sort. And since we cannot specify which movements *must* be involved in attaining the goal, so also we cannot specify *precisely* which antecedent movements are sufficient to initiate behaviour. This general logical difficulty holds against Hull's more complicated theory as well as against Hobbes' simpler one.

This kind of logical difficulty is even more glaring in Hobbes' theory of the passions. For most of our terms at this level of description are either like ' ambition ' in assigning a certain kind of objective to an action or like ' honesty ' in classifying an action as being in accordance with a certain rule or convention. It is most unpalatable to suggest, as Hobbes did, either that such terms imply anything specific about the efficient causes which initiate behaviour of this kind,² or that such behaviour could be *deduced* from a theory concerned only with colourless movements. For a gross muddle of explanatory models is involved. Terms like ' ambition ' and ' honesty ' derive their meaning from a model of behaviour peculiar to goal-directed and rule-following activities, which is of quite a different logical type from that of mechanics. In this explanatory model an agent is assumed to have an objective (like being a professor, in the case of ' ambition '), and to have information about means which will lead to this objective in a manner which is both efficient and in accordance with certain conventions of appropriateness (as in the example of ' honesty '). This model forms a kind of explanatory ceiling in understanding human behaviour just as the mechanical model of bodies pushing other bodies formed an explanatory ceiling in the seventeenth-century understanding of nature. And all our psychological explanations are related to this model just as all explanations in classical economics presupposed the model of a rational man.

Now physiological descriptions can state *necessary* conditions for behaviour conforming to this model ; for it is a truism to say that we

¹ T. Hobbes, *E.W.*, Vol. I, p. 132

² See R. Peters, *Hobbes*, Penguin Books, 1956, pp. 144-147

cannot plan means to ends or be sensitive to social norms unless we have a brain. Similarly physiology, like psycho-analysis, can state conditions under which this type of behaviour breaks down. A man with a brain injury may well be insensitive to social pressures just as a man with an obsession may be incapable of taking the means necessary to bring about a desired objective. Obviously physiological theories are extremely *relevant* to explanations of action at the molar level of behaviour. But this does not mean that there is a *deductive* relation between them—that behaviour can be deduced from the physiological description *alone*. Our contention is that Hobbes and Hull were mistaken in assuming that the relation was of this sort.

But surely, it might be objected, Hull had much more rigorous standards of scientific method than Hobbes. Surely he must have introduced subsidiary hypotheses to bridge the gap between physiological and psychological descriptions. On the contrary, our case is that neither Hobbes nor Hull saw that these types of explanations were of logically different types. Hull's ultimate aim is a 'truly molecular theory of behaviour firmly based on physiology.'¹ As this is at present impossible because of the inadequacy of our knowledge, a molar approach based on the use of 'quasi-neurological principles' must serve for the time being. There are, however, 'degrees of the molar, depending on the coarseness of the ultimate causal segments or units dealt with. Other things equal, it would seem wisest to keep the causal segments small, to approach the molecular, the fine and exact substructural details, just as closely as the knowledge of that substructure renders possible.'²

This makes explicit Hull's assumption that the difference between physiology and psychology is only a difference in the 'coarseness of the ultimate causal segments or units'. There is no *logical* difference, on his view, between these explanations; it is merely a matter of the 'fineness' of the 'substructural details'. Yet as soon as he starts developing explanations instead of just making programmatic pronouncements, the logical gulf immediately appears. For instance, as Koch points out, 'stimulus' is conceptually defined by Hull either in terms of physical energy, or in terms of neural impulses. R is 'reaction or response in general (muscular, glandular, or electrical)'; but when Hull refers to stimuli or responses in his description of the behaviour of experimental rats, R comes to mean *actions* such as 'biting the floor bars',

¹ Hull, *op. cit.*, p. 20

² *ibid.* p. 21

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'leaping the barrier', and so on.¹ Stimuli, to quote Koch again, 'are certainly not being specified in terms of independent physical energy criteria'. The symbols which previously referred to the 'substructural detail' are kept unchanged, but even at this low level of behavioural complexity, they acquire new meanings: they refer to *actions* classified in terms of their end-results.

This reference to the 'substructural detail' also occasions another query. What sort of description is appropriate to it? Is it in fact described in physiological terms? Or could it be that Hull, like Hobbes, makes a plausible transition from physiology to psychology by according the 'logically more primitive elements' a sort of twilight status? Hobbes found the elements on which he constructed his system in motions of particles of all sizes. When a jump into the unobservable became necessary, motions became shadowy 'endeavours' which belonged to minute particles of matter. The 'reality status' of minute motions in Hobbes' system was obvious and explicitly affirmed. Hull's position, however, is more ambiguous. The data for both sides of his formulae are stimuli and responses, or molar movements of the organism. Between these two classes of observables, a series of 'theoretical constructs' serves the attempt to express the infinite variation at both ends in some uniform, lawful, and communicable manner. The constructs are not meant to be observable, and are, or should be, unequivocally defined without reliance on 'substance'. Discussions about the doubtful status of these supposedly abstract links are a familiar feature of the recent psychological literature, and need not be invoked here in detail.² The main objection levelled against them is that they are not abstract, but have an implicit existential status. 'Habit strength' may well be an abstract quantifiable concept, but 'habit' or 'reaction potential' are for Hull not only theoretical constructs. They are also 'neural organisations', they form pseudo-physiological 'pictures' of what happens inside the organism. These events are described, as in Hobbes' system, in terms of minute motions.

¹ S. Koch, *Clark L. Hull in Modern Learning Theory*, New York, 1954, pp. 24-25

² See, for example, F. H. George, 'Logical Constructs and Psychological Theory', *Psychol. Rev.*, 1953, pp. 1-6; S. Koch, *Clark L. Hull in Modern Learning Theory*, New York, 1954; K. MacCorquodale and P. E. Meehl, 'On a Distinction between Hypothetical Constructs and Intervening Variables', *Psychol. Rev.*, 1948, **55**, 95-107. Koch's paper especially contains a very detailed discussion of the logical difficulties raised in Hull's system by the ambiguous, pseudo-physiological character of the 'theoretical constructs'.

And just as Hobbes' 'endeavours' enabled him to slip unobtrusively from mechanical to psychological descriptions, so also Hull's language shuffles between that appropriate to a description of the physiology of the central nervous system and that which is used to describe observable molar events. But it is not definitely committed to either. A peculiar use of terms (e.g. 'reaction-potential') bridges the gap in both systems: language describing the 'primary elements' is still used in the description of behaviour, and the transition is achieved because its difficulties are ignored.

It is this which renders untestable an important aspect of Hull's theory. System-builders who aim at an 'explanation of human behaviour' and find their point of departure in any form of atomism must state clearly the steps which enable them to hope for such an achievement. It is true that many of Hull's hypotheses have been tested in a number of severely limited experimental situations. Indeed it is often said that testability is one of the main virtues of Hull's theory; for he was 'the first psychologist who could be proved to be wrong'.¹ But these tests only establish certain regularities of behaviour in extremely simple situations without showing how these regularities can be deduced from the underlying principles of internal motion. Neither do these tests in any way establish the applicability of such simple laws to forms of behaviour such as are outlined in his ambitious scheme which we have described above.

4 *Consciousness and Rational Thought*

If we can trust Hobbes' autobiography, his psychology was developed in part as an answer to a problem that haunted him for years. He had once been present at a gathering of learned doctors who were discussing problems connected with sensation. One of them asked what, after all, sensation was, and how it was caused. To Hobbes' astonishment not one of them was able to suggest an answer. Hobbes pondered over this for years until, after his meeting with Galileo, a solution suddenly occurred to him. He looked at the familiar process of sensation in the unfamiliar way he had learnt from Galileo

. . . it occurred to him that if bodies and all their parts were to be at rest, or were always to be moved by the same motion, our discrimination of all things would be removed, and (consequently) all sensation

¹ Derek Pugh, Review of *A Theory of Social Control*, *British Journal of Psychology*, 1955, 46, 153

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with it ; and therefore the cause of all things must be sought in the variety of motion ¹.

Sensation, which was but ' some internal motion in the sentient ', was a meeting place of motions. Deductions from a general mechanical theory were all that were required both to explain the peculiarities of sensation itself and the initiation of actions in response to external stimuli. These Hobbes proceeded to provide.

The selectivity of perception was explained by suggesting that while the organ retains motion from one object, it cannot react to another ; similarly in attention the motion from the root of the nerves persists ' contumaciously ', and makes the sense-organ impervious to the registering of other motions. The explanation of imagination is a straight deduction from the law of inertia :

When a body is once in motion, it moveth, unless something else hinder it, eternally ; and whatsoever hindreth it, cannot in an instant, but in time, and by degree, quite extinguish it ; and as we see in the water, though the wind cease, the waves give not over rolling for a long time after ; so also it happeneth in that motion, which is made in the internal parts of man, then, when he sees, dreams, etc. . . . Imagination therefore is nothing but decaying sense.²

The decay, of course, is not a decay in motion. For that would be contrary to the law of inertia. Rather it comes about because the sense-organs are moved by other objects. This explains the vividness of dreams. For in sleep there are no competing motions from the external world. When sense-impressions are constantly crowding in on us, the imagination of the past is obscured and ' made weak as the voice of a man in the noise of the day '. Thus the longer the time that elapses after sensing an object, the weaker our imagination.

There is something almost incredibly hard-headed and naive about Hobbes' gross materialism. To say that sensation and the conceptual processes are *nothing but* motions is rather like saying that kissing is simply a mutual movement of the lips or that work is moving lumps of matter about. Hobbes, too, is aided in this rather monstrous piece of metaphysics by using terms like ' agitation ', ' celerity ', ' disturbance ', and ' tranquillity ' to describe mental processes ; for these terms have meaning as descriptions both of physical and psychological happenings. Hobbes could thus talk like a physiologist and preserve the common touch of everyday psychological description. But at any rate he did

¹ T. Hobbes, *L.W.*, Vol. I, p. 21

² T. Hobbes, *E.W.*, Vol. III, p. 4

openly, not to say brazenly, make the transition from mechanics to psychology. He did not, however, seem to be sufficiently aware of the *sort* of gap that he is bridging. For just as he developed a *causal* theory of imagery and thought also that he was answering questions about the reference or *meaning* of images, so also he thought that differences between activities like perceiving, imagining, and remembering could be explained solely in terms of their efficient causes. But the distinction between sense and imagination is not *simply* that imagination is *decaying* sense any more than the distinction between imagination and memory is that the latter involves only the addition of a sense of pastness. For these activities have different names because they imply different logical criteria. Psychologically speaking perceiving may be the same as imagining in a given case. When we say, in spite of this that we did not *imagine* something, we are making a logical point, not a psychological one. Human actions imply criteria of distinction which are at quite a different logical level from that appropriate to stimuli, movements, and other such mechanical concepts.

Hobbes, then, leapt openly, if recklessly, from mechanics to psychology. Hull, who deals very little with sensation, either ignores the gap or bridges it by implied assumptions. He ignores problems connected with the status of consciousness and his assumptions about sensation are implicit in his development of a theory of learning rather than explicitly stated. Hobbes assumed that identical motions from the external world will lead to identical counter-motions in the organism ; in other words, discrimination between various stimuli, and generalisation of responses to stimuli varying quantitatively and qualitatively will be a function of the degree of difference between the motions imposed on the organism from the outside. Hull, preoccupied with learning rather than with problems of perception, is interested only in the influence of past events on present perception. But again the conclusions reached in both systems are almost identical, as the common assumption is that, in the last analysis, it is the degree of identity of patterns of minute motions which determines the degree of identity of perceptions. And no doubt there are such differences in minute movements. But it is questionable whether a knowledge of these alone would ever enable us to explain the differences in the descriptions of what we see. For such descriptions involve the use of conventions and standards of correctness which we impose on what we see. Man is a rule-following animal in perceiving as well as in moral behaviour, and

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it is this characteristic which makes all such causal theories unpalatable as *sufficient* explanations of his activities.

Hobbes saw that it was man's capacity for using symbols in deductive reasoning and in descriptive languages which distinguishes him from animals, together with the theoretical curiosity that goes along with it. But he even suggested a mechanical explanation of language in his crude causal theory of signs. This was a grotesque failure because he never properly distinguished logical questions of the reference of signs from causal questions of their origin. Similarly he gave a mechanical explanation of choice. Will, he held, simply is the last desire in deliberating which emerges after an oscillation of impulses. Here again, in his writings on free-will, he never properly distinguished questions about the justification of actions (their reasons) from questions about their causes. Indeed, he seemed to think that *all* reasons for actions are rationalisations—a smoke-screen concealing the underlying thrust and recoil of a pleasure-pain calculating machine. But this is inadequate. For there is a manifest difference between compulsive and rational behaviour. A person who deliberates rationally about means to an end will be influenced by logically relevant considerations. For him there is a difference between good and bad reasons for a course of action. But for a compulsive there is no such similar distinction. No reasons will make any difference to what he does. Like a man under post-hypnotic suggestion he will only 'reason' to find excuses for what he is going to do anyway. Now any mechanical theory, even if it has recourse to minute motions, must face the glaring inappropriateness of giving causal explanations of transitions in terms of logical dependence. In what sense can a physiological theory of the brain be said to *explain* a geometer's conclusions or a move at a game of chess?

Hull suggests in his opening chapter that all sorts of formalised procedures like those of law, ritual, and government, can be explained by means of his mechanical theory. But, needless to say, he never gives an inkling of how this can be done. Is there much point in elaborating a system in such detail and making such far-reaching claims for the derivations which one day might be made from it, if the grave logical problems of applying such mechanical explanations to distinctively human behaviour are completely ignored? Hobbes saw the crucial gaps and audaciously, if unconvincingly, attempted to leap them. Could it not be said that the detail and alleged logical rigour of Hull's system, far from putting psychology on a truly scientific

path, merely serve to conceal important logical difficulties in his system ?

In his last book Hull wrote :

It is clear from the foregoing discussion that natural-science methodology presumably will be able, ultimately, to deduce from its principles all kinds of behavior of organisms, whether generally characterised as good, bad, or indifferent. Moreover, since the passing of a moral judgement is itself a form of verbal behavior, either overt or covert, it is to be expected that natural-science theory will be able to deduce the making of moral judgements along with other forms of behavior.¹

Now it is understandable that Hobbes should also have shared this methodological pipe-dream ; for he lived before Hume and Kant had shown the logical impossibility of deducing statements about what ought to be from statements about what is the case. But any modern philosopher, who read this extract from Hull, would marvel at the naiveté of a man who thought that normative judgments could be deduced from a physiological theory. Our case, however, has not been a laboured exposition of this obvious logical lapse. It has been, rather, to stress that the logical leap occurs in a much more interesting transition—in that from movements to actions. Misled by the obvious fact that physiological theories are extremely *relevant* to explanations of human actions, Hull, like Hobbes, thought that descriptions of human actions could be *deduced* from a physiological theory alone. This, in our view, is the basic logical mistake in mechanistic theories which both Hobbes and Hull commit in a surprisingly similar manner.

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¹ T. Hobbes, *E.W.*, Vol. III, p. 338

A COMPARISON OF PROCESS AND NON-PROCESS THEORIES IN THE PHYSICAL SCIENCES*

BRIAN ELLIS

I

IN giving an explanation we often describe a process which enables us to see whatever it is to be explained as a necessary consequence of its operation. That is, we attempt to answer the question, 'What is *happening* that something or other should be the case?' I shall call an explanation of this kind a *process* explanation. Sometimes, however, no attempt is made to answer such a question. All the processes involved in the phenomena to be accounted for are treated as though they were in no need of further analysis. The explanation merely shows us how apparently disconnected events can be seen as the effect of the operation of a single principle. Such an explanation will be described as a *non-process* explanation.

Examples of both process and non-process explanations are readily available in the physical sciences. The explanation of gravitational phenomena which Newton gave in the *Principia* is of the non-process kind, for it makes no attempt to say how one body succeeds in attracting another. Attraction and repulsion are treated as fundamental and irreducible processes—on the same plane as, say, the process of moving from one point to another. The question, 'How do bodies attract each other?' is simply not considered. Newton's explanation does, however, enable us to see the various gravitational phenomena as due to the operation of a single principle. For these reasons it may be described as a non-process explanation. On the other hand, almost any explanation given on the molecular theory of gases is a process explanation. Thus Avogadro's explanation of Gay-Lussac's Law of Combining Volumes¹ proceeds by saying how, in general, gases unite to form compounds. In other words it describes a process which is said to be the process of chemical combination.

* Read to the Philosophy of Science Group, January 1956

¹ Gay-Lussac's Law of Combining Volumes states that when gases combine they do so in simple proportions by volume, the volume of the product, if gaseous, bearing a simple ratio to the volumes of the reactants when measured under the same conditions of temperature and pressure.

Gay-Lussac's law is now seen as a necessary consequence of the operation of this process.

My aim in this paper is to compare these two kinds of explanation and to say what reasons there are for preferring one to the other. To avoid difficulties arising from differences of subject-matter, I have taken as examples a process theory, which, incidentally, has already received universal acclaim, and a non-process theory which is designed to account for precisely the same facts. On both theories it is possible to explain certain laws of chemical combination in gases, and, in particular, the fact that one volume of hydrogen combines with one of chlorine to form two of hydrogen chloride. The immediate difference between the two, which is the reason for calling one a process theory and the other not, is that one begins by reducing the process of chemical combination to one of rearranging elementary particles, while the other simply treats the process of chemical combination as fundamental, in much the same way as Newton's theory treats gravitational attraction.

Here, then, are the two theories.

The Process Theory: (Theory A)

In order to explain the fact that one volume of hydrogen combines with one of chlorine to form two of hydrogen chloride, a number of hypotheses have to be, or have been made. On Avogadro's theory these will include presuppositions about the nature of gases in general and the way in which chemical combination takes place, Avogadro's hypothesis, and further hypotheses about the natures of the gases involved in this particular reaction. The hypothetical system necessary to deduce the fact to be explained is :

(i) That each gas is entirely composed of small identical particles which are said to be molecules of that gas—the Molecular Hypothesis. (This hypothesis is roughly equivalent to Dalton's Atomic Hypothesis, but we shall call it the 'Molecular Hypothesis' in accordance with modern usage.)

(ii) That molecules are entirely composed of smaller particles which are called atoms. And that molecules of each element are entirely composed of atoms of *one* kind which are said to be atoms of that element, and molecules of compounds are entirely composed of atoms of *various* kinds which are atoms of the elements which go to make up the compound—the Atomic Hypothesis.

(iii) That chemical combination between gases is brought about by molecules of one gas coming together with molecules of another gas

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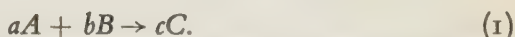
forming new molecules (of the gaseous compound) by a regrouping of their constituent atoms ; this process being repeated throughout the gases—the Process Hypothesis.

(iv) That equal volumes of all gases under the same conditions of temperature and pressure contain the same number of molecules—Avogadro's Hypothesis

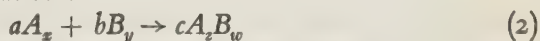
(v) That hydrogen and chlorine are both diatomic molecules and that a molecule of hydrogen chloride consists of one atom of hydrogen and one of chlorine—the Particular Hypothesis.

Adequacy. From this set of hypotheses it can be deduced that one volume of hydrogen combines with one volume of chlorine to form two volumes of hydrogen chloride. This will be expressed by saying that the set is *adequate* for the deduction of this fact. That this set of hypotheses is adequate has now to be established.

The third hypothesis says that a chemical combination of gases may be described in terms of molecules of one gas meeting molecules of the other, regrouping their atoms and forming molecules of the product gas ; so that when gases *A* and *B* combine, the elementary process which is repeated throughout the gas is that *a* molecules of *A* combine with *b* molecules of *B* to form *c* molecules of *C*. If I write *aA* to mean ' *a* molecules of *A* ', *bB* to mean ' *b* molecules of *B* ', *cC* to mean ' *c* molecules of *C* ', ' + ' to mean ' combine with ' and ' → ' to mean ' to form ' ; then I may translate this by a rudimentary chemical equation :



If *A* and *B* are chemical elements I can write *A_x* to mean ' *A* which is *x*-atomic ' and *B_y* to mean ' *B*, which is *y*-atomic ', and since *C* is a compound of *A* and *B*, I can write *A_zB_w* for ' *C*, a molecule of which contains *z* atoms of *A* and *w* atoms of *B* '. This enables me to write down the schematic equation :



which may be read : ' *a* molecules of *A* which is *x*-atomic combine with *b* molecules of *B* which is *y*-atomic to form *c* molecules of *C*, a molecule of which contains *z* atoms of *A* and *w* atoms of *B* '.

The first three hypotheses, therefore, are all that are required to set up a schematic chemical equation (a sentence schema), values of which would describe the supposed atomic-level processes of chemical combination. And since *A* and *B* may refer to any gaseous elements which combine to form a gaseous compound *C*, the above sentence

schema may be turned into a description of the supposed atomic-level process of any such combination. Hence the Atomic, Molecular, and Process hypotheses provide us with a schema for accounting for this type of chemical union. They provide us with a statement formula, the instances of which are descriptions of certain non-observable processes of chemical combination. I do not know whether it is necessarily true that theories should provide such sentence schemata, but I suspect that it is, and that *to give an explanation on a theory* is simply to give an appropriate evaluation of a sentence schema which the theory provides.

This statement formula (2), has seven numerical variables and two substantival variables. However, they are not all independent. The total number of atoms of each element taking part in an atomic-level process of chemical combination must equal the total number of atoms of each kind in the compound molecules so formed, i.e.

$$ax = cz \tag{3}$$

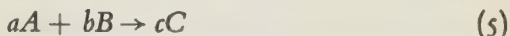
$$\text{and} \quad by = cw \tag{4}$$

for by hypothesis the combination process is *rearrangement* of the atoms in the combining molecules, the new groups being molecules of the compound. These relations reduce the number of independent numerical variables to five, and, given a specific chemical union of gases to account for, these five variables are all that remain to be fixed.

The remaining two hypotheses are best seen as successive stages in determining these remaining variables. If we are interested in the proportions by volume in which the gases combine to form a compound, then by Avogadro's hypothesis we should be interested only in the *relative* number of molecules taking part in, or being formed by, an atomic-level process. This reduces the number of numerical variables to four by allowing us to treat the ratios a/b and b/c as single variables, thus replacing three variables by two. The Particular Hypothesis then fixes the remaining four numerical variables and three substantival variables by putting $x = y = 2$ and $z = w = 1$, and A , B , and C equal to hydrogen, chlorine, and hydrogen chloride respectively. From this set of hypotheses, therefore, one can readily calculate that $a : b : c = 1 : 1 : 2$ and, therefore, that the volume of hydrogen is to the volume of chlorine is to the volume of hydrogen chloride as $1 : 1 : 2$. Hence it may be said that the set of hypotheses (i), (ii), (iii), (iv), and (v) form an adequate set for the deduction of this fact, and that Avogadro was successful in accounting for the volume law for the combination of hydrogen and chlorine.

The Non-process Theory : (Theory B)

The various facts which gave rise to the Gay-Lussac Law may be listed. One such fact would be that one volume of hydrogen combines with one volume of chlorine to form two volumes of hydrogen chloride. This suggests the following formulation of the Gay-Lussac Law : a volumes of gas A combine with b volumes of gas B to form c volumes of gas C ; a , b , and c being small positive integers (seldom greater than 3) whatever the gases A , B , and C may be, provided that these gases A and B do combine to form a compound C . If we agree to write aA for ' a volumes of A ', bB for ' b volumes of B ', cC for ' c volumes of C ', and '+' for 'combine with' and ' \rightarrow ' for 'to form', the Gay-Lussac Law may be restated symbolically :

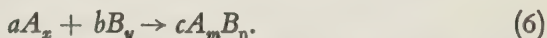


where a , b , and c are small positive integers whatever gases A and B unite to form a gas C . Since the gas C is a compound of A and B , this can be simplified by writing $A_m B_n$ in the place of C , where m and n are numbers the determination of which depends on the choice of A and B and the way in which they are combined.

As a first step towards explaining the law of combining volumes of hydrogen and chlorine I make the hypothesis :

That each reactant elemental gas has a certain property which I call its 'gas number'. The symbol A_x will be used to mean 'the gas A , whose gas number is x '.

On this hypothesis a particular combination of gases A and B may be described :



This sentence schema is the same as the sentence schema (2). It has two independent substantival variables and seven numerical variables. I said that I would make the numbers m and n depend upon the choice of A and B and the way in which they are combined. Hence I may define m and n by the following two relations :

$$ax = cm \quad (7)$$

$$\text{and} \quad by = cn. \quad (8)$$

There is no *a priori* reason for doing this in this particular way. But it is as simple as any, and in any case it was just such a simplifying hypothesis which, though wrong, made Dalton's original atomic theory so useful for scientific purposes.

These definitions reduce the number of independent variables to

five. But since Gay-Lussac's Law concerns only the *proportions* by volume in which gases combine to form compounds, only the ratios a/b and b/c matter. This effectively reduces the number of independent variables to four.

In order to account for the particular chemical combination of hydrogen and chlorine with which we began, one further hypothesis has to be made, viz :

That hydrogen and chlorine both have the gas number 2, and that

$$m = n = 1.$$

This hypothesis is the non-process theory's equivalent of the 'Particular Hypothesis' of Avodagro's System. It succeeds in fixing the remaining four numerical variables, thus making it possible to calculate that the volume of hydrogen is to the volume of chlorine, is to the volume of hydrogen chloride as 1 : 1 : 2. Hence this set of hypotheses is also adequate for the deduction of this fact.

2

Discussion and Comparison of the Two Theories

(1) As far as the logical rôles of these theories are concerned, there is nothing to choose between the two. First, the hypotheses of both are such that they enable us to deduce, in a strictly logical way, that one volume of hydrogen combines with one of chlorine to form two of hydrogen chloride ; but the reverse is not the case, viz. that the hypotheses can be deduced from this law. Secondly, both theories have, so to speak, the same number of degrees of freedom, i.e. there are the same number of variables to be fixed. Moreover, these variables would be fixed in precisely the same way. The way in which we should determine the atomicity of hydrogen would also serve to determine its gas number. Thirdly, both theories enable us to give a higher order redescription of the phenomenon to be explained. Values of the sentence schemata (2) and (6) are such redescriptions. In these respects, then, the theories are equivalent ; so that unless there are other logical rôles of theories and explanations (some would say, for instance, that a theory *must* say *how* a process takes place), we cannot express a preference for one or the other of the two theories on logical grounds alone.

One argument against the non-process theory might be that its acceptance would upset an otherwise neat theory of explanation which demands that all explanations be given in terms of attractive and

repulsive forces between elementary particles and their consequent movements. For if the explanation were accepted it would mean placing chemical combination in gases on the same plane as attraction and repulsion, thus creating a new theory of explanation. The new theory would be that explanations must be given in terms of attraction and repulsion *and* chemical combination in gases.

But many people would say that the particular theory of explanation which the acceptance of the non-process theory would destroy, has already been outdated by wave-mechanics, and that in any case they do not believe in theories of explanation. Some scientists put this last point by saying that they 'do not believe in explanation'. But this is somewhat paradoxical for they continue to speak of explaining things. When someone gives a non-process explanation which, say, treats chemical combination as a fundamental process, he is not demanding that all explanations should treat it likewise. What is true is that if one believes that all things are finally explicable in terms of certain fundamental processes, then to have to accept an explanation such as that given on Theory B is upsetting in that we should have to postulate more fundamental processes than are logically necessary. But many scientists do not believe that there are any such fundamental processes.

Next it may be said that the non-process theory is not a theory at all, that nothing is explained by it, because it does not make the reaction of gases with the consequent formation of compounds any more intelligible. But the notion of intelligibility does not necessarily coincide with that of offering a useful scientific explanation. Neither Newton's Theory of Gravitation nor Maxwell's Theory of the Electromagnetic Field were felt to be 'intelligible' in this sense. So we cannot rule out the second theory *a priori* in this way. If our preference is for the first, we shall have to say just what it is about it which, from an empirical point of view, makes it potentially the better theory (if it is), for it is one of the objects of this paper to rationalise our selection of scientific theories, not to make it dependent on such illusory notions as their innate satisfactoriness. We shall, in other words, have to say in which empirical rôles it is potentially the more efficient theory, and why.

(2) One empirical rôle of a theory is to enable us to make predictions. From this point of view, Avogadro's theory appears to be the better of the two. Given even the most elementary knowledge of the theory of probability, we should be able to predict that when gases combine they do so in simple proportions by volume. For if

chemical combination takes place by a chance meeting of molecules of different kinds, it is most unlikely that such a process would involve large numbers of molecules. On the non-process theory, however, we could not make this prediction. The most we could do, by way of explanation of Gay-Lussac's Law, is say that the simplest assumption is that the gas numbers x , y , m , n are small positive integers. One advantage of the process theory is, then, that it reduces the process of chemical combination, about which we know little, to a process involving the movements of particles, about which we know a great deal, and this enables us to say some things about the process of chemical combination which we could not do otherwise. There is no comparable lever for prediction in the case of non-process theories.

However, it does not follow from this that non-process theories are no good for prediction. There is more than one way to Old Sarum. There may, for example, be formal analogies between theories in different fields, and this may serve as a basis for predicting that relations which hold in one field will hold in the other. Any physicist is familiar with certain formal analogies between portions of hydrodynamics and electrical circuit theory. Again, there may be phenomena which have an obvious interpretation in the terms of a non-process theory, and this may help us to make certain predictions. For example, perturbations in the orbit of Neptune had a fairly clear interpretation within Newton's Theory of Gravitation. The existence of Pluto was successfully predicted. All we are entitled to say is that if we employ process theories, then we have an *additional* lever for prediction.

Another empirical rôle of a theory is to suggest lines of research beyond the mere confirmation of the law or laws the theory is designed to explain. For this reason one theory may be potentially more useful than another. In the case we are considering, the process theory raises many problems. How many molecules are there in a given mass of gas? How do the atomic-level processes of chemical combination take place? How are the atoms arranged within the molecules? What is the nature of the forces which hold them together? Why is hydrogen diatomic, not triatomic? In the non-process theory, however, there seems to be no equivalent of any of these problems. The only comparable question is, 'Why do gases have gas numbers?' And the process theory provides us with the answer. It may be said, therefore, that the process theory raises more questions than its rival, and this may be taken as meaning that as a matter of fact it suggests more lines of research.

However, this conclusion does not follow automatically. For although more questions seem to need asking, it may not be true that these questions suggest further lines of research. Thus we may ask how many molecules there are in a given mass of gas. But this question, by itself, certainly does not suggest, say, Millikan's oil drop experiment by which the number may be determined. Indeed if, according to supposition, molecules are unobservable, and those of a particular kind are alike in all respects, then they must be indistinguishable, and we may ask how we could ever know how many there are in a given mass of gas. The same holds for the other questions. We do not even know what it would be like to make a discovery about the supposed atomic-level processes of chemical combination unless we are supplied with considerably more information. So far, then, are we from *planning* to make such a discovery. What lines of research are suggested by the question: 'How are the atoms arranged within the molecule?' In Avogadro's time this question lacked a sense. For nobody could have said what it would be like to find out that the atoms were, say, fused together, or rotating about each other, or arranged concentrically. How do we go about answering the question: 'What is the nature of the forces which hold the molecules together?' Where do we start if we want to answer the question: 'Why is hydrogen diatomic, not triatomic?'

It would be absurd to argue from this, however, that theories do not act as guides to research. Indeed if they were empty and suggestionless they could never be refuted. The Michelson-Morley experiment, for example, must have been suggested by the theory of the æther. All that this argument shows is that if you just look at the questions raised by a theory, assuming no knowledge of its extensions, often no guidance to research can be obtained. What I have overlooked is that phenomena in the other fields may have an obvious interpretation in the terms of this theory, and this in turn may provide us with an interpretation of some of the questions raised by the original theory. An example will bring out this point.

In the electrolysis of water, hydrogen is liberated at the cathode and oxygen at the anode. We know that like charges repel and unlike charges attract each other. Hence it is reasonable to say that the hydrogen atoms in water are positively charged and that each atom carries the same charge. Here, then, is an example of a phenomenon having an obvious interpretation in the terms of the atomic theory. It is found that the same quantity of hydrogen is always

liberated by the same quantity of electricity. Therefore we may say that whenever a hydrogen atom occurs in a compound it always carries the same positive charge, and for convenience we may describe it as an 'ion'. Given this information, the question raised by the original version of the molecular theory of how many molecules there are in a given mass of gas, acquires a new significance. For if we can measure the charge on a single ion, we can easily calculate how many ions must be neutralised in making up any given quantity of gas, and hence calculate how many molecules there are in the given quantity. The question thus becomes: 'What is the charge on a single hydrogen ion?'

Hence the fact, if it is a fact, that the process theory raises more questions than its 'rival' may yet be an important factor in the comparison we are making. That the questions may have no obvious interpretation seems to be no handicap. On the contrary, it is in finding interpretations of them, and attempting to answer the interpreted questions, that much progress in science consists. A theory which raised no questions could not take part in such progress. This, then, may be a good reason for preferring the process theory.

But can it be shown that the process theory raises more questions than the non-process theory? As a first step in this direction it should be observed that any question about gas numbers raised by the non-process theory can be translated into the terms of the process theory by substituting the phrase, 'number of atoms per molecule' for 'gas number'. Thus, 'Are gas numbers in any way related to density?' may be translated 'Are numbers of atoms per molecule in any way related to density?' and 'Is the gas number of a gas calculable from its atomic weight or valency?' may be translated in a similar way. Indeed, any question raised by the Theory B about gas numbers may be converted into a question about numbers of atoms per molecule by this simple substitution, and for every such question there is a question raised by the Theory A.

Likewise questions (other than analytic) of the Theory B about the numbers m and n can be translated into questions raised by the Theory A by the substitution of the phrase 'number of atoms of A in the compound molecule' for 'number n '. It appears, therefore, that any question raised by the Theory B can be translated into a question raised by the Theory A. For the only new terms introduced into the non-process theory are 'gas number' and 'numbers m and n ', and no other new objects, processes, events, or properties are

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described. Since any other questions which might be said to be about the theory could be asked without a knowledge of it (the questions not being about anything with which we might not otherwise be familiar), it follows that they cannot be questions raised by the Theory B. Now, we have already seen that there are many questions raised by the Theory A which cannot be translated into the terms of the Theory B. Hence, it may be said that A has a greater question-raising potential than B, and possibly this gives us some reason for saying that it will be more useful for scientific purposes.

However, many have been quick to point out that this may not be the case. They argue that much time and energy is wasted in trying to answer questions to which there is no answer. Duhem¹ reasons in this way. But it is difficult to support this view by reference to the history of science. Unexpected negative results have often been as fruitful, and indeed more fruitful, in promoting conceptual advances than expected positive ones. It does not seem to me that the Michelson-Morley experiment was just so much time wasted in trying to substantiate a theory which need never have been put forward in the first place.

So far, most of my arguments have tended to favour the process theory. But to stop at this point would be to give only part of the picture. For to express a preference for process theories is to suggest that it is quite in agreement with scientific method to allow unnecessary unobservables in the formation of a theory. It was not, for example, necessary to postulate the existence of atoms and molecules and para-mechanical processes of chemical combination in order to account for the volume law of gas reactions.

This suggestion is certainly paradoxical. The paradox can be partly removed if I say in what sense the molecular hypothesis is 'unnecessary' for the explanation of Gay-Lussac's Law of Combining Volumes. The molecular hypothesis is unnecessary only in the sense that it is possible to construct another theory which does not employ it, but which has the same logical function, and which is such that it is not possible to find a 'translation' for the term 'molecule' in its terms. It is not, however, unnecessary in the sense that it is superfluous to Avogadro's explanation. We could not, indeed, have any of the other hypotheses of his theory without it. But the conclusion remains that if we would accept Theory A, rather than B,

¹ Pierre Duhem, *The Aim and Structure of Physical Theory*, Princeton, 1954

without further confirming evidence, then we must admit that it is in agreement with scientific method to allow (in this less offensive sense of 'unnecessary') unnecessary unobservables in the formation of a theory, and this is undoubtedly an argument in favour of the non-process theory, because from the point of view of the number and complexity of the hypotheses which are made it is the *simpler* theory.

(3) In conclusion, there are good reasons for preferring process theories when they can be given. Not only are they more likely to enable us to make predictions in the field we are investigating, they are more likely to lead to extensions of our knowledge into other fields, and to suggest further lines of research. But, on the other hand, non-process theories are not simply to be discounted. In most empirical rôles process theories are superior, but this is not to say that non-process theories are useless. Indeed, in some respects they have advantages. For instance, a non-process explanation can often be given where a process explanation does not seem to be possible. Non-process explanations are not tied down to processes, and for this reason their field is wider. Metaphorically speaking, non-process explanations may take over where process explanations leave off.

The first of these conclusions is hardly a novelty. Campbell said more than thirty years ago that 'Mechanistic' theories are more useful 'because they say more'.¹ But his statement is left at this metaphorical level. He does not tell us anywhere what he means by what a theory 'says'. And as there are a number of things which he might have meant, it is highly ambiguous. Throughout this paper I have tried to avoid this improper use of metaphor, to which philosophers of science seem particularly prone, and to state my conclusions unambiguously. The conclusion that non-process theories must not be simply ruled out *a priori*, seems to be of more consequence. For many have wanted to say that non-process theories are mere skeletons of theories, and completely unworthy of the name. Such views arise, I suspect, because we are inclined to consider only some of the empirical rôles of theories and explanations in science, and even then to overlook the fact that there is, in general, a variety of ways in which a theory may play its part efficiently in any given rôle.

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¹ N. R. Campbell, *Physics: The Elements*, Chap. VI

DISCUSSION

SOME ASPECTS OF PROBABILITY AND INDUCTION: A REPLY TO MR. BENNETT

In the numbers of this *Journal* for November 1956 and February 1957 Mr Jonathan Bennett has examined certain parts of my book *Probability and Induction* and drawn attention to several points on which he thinks I am 'fairly clearly mistaken'. Two of his criticisms are well-founded, but I believe that the others rest on misunderstandings, and I hope to show this by discussing his various arguments in turn.

1 *The Consilience of Inductions*

(i) Under this heading Mr Bennett objects first to a statement on page 107 of my book that a 'biological generalization cannot . . . be less probable than the physical and chemical laws from which it is seen to follow'. He says that a biological generalisation never follows simply from physical and chemical laws, but only from these taken together with some minor premiss, or bridging proposition, about the physical and chemical make-up of the organisms under investigation, and that 'this fact lets in a possibility of error which puts the generalisation to be explained on an entirely different level from the generalisation explaining it'.

I agree that what I said in this place about one generalisation's following from another was loosely worded, and that a biological generalisation which we try to explain by reference to physical and chemical laws may well remain less probable than those laws if a minor premiss involved in the suggested explanation is itself less probable than the laws. But the dots at the beginning of Mr Bennett's quotation from me (not reproduced here in full) mark his omission of some words linking the passage with the previous section in which I wrote explicitly of deducing generalisations about organisms, not from more general laws considered alone, but from these taken together with propositions about the constitution of the organisms. When I said in the passage quoted that a biological generalisation could not be less probable than physical and chemical laws from which it was seen to follow, I was assuming for the moment that the relevant proposition of constitution might be taken as certain. No doubt I was wrong in speaking only of this limiting case; but in biology some propositions of constitution are taken as certain beyond all question, although there seems to be no difference of kind between them and other propositions of constitution which are treated as empirical generalisations subject to some doubt.

Mr Bennett overlooks this last point when he says that the biologist who tries to explain biological generalisations in terms of physical and chemical laws must produce not definitions but hard-won discoveries about the physical and chemical make-up of his material. If a natural historian first remarks with some hesitation that mice when dropped from a height fall slower than men, and then explains his generalisation by the reflection that mice, being smaller, offer a greater surface for

air friction in proportion to their volume, the minor premiss on which he relies is surely a truth that we all learnt in learning to use the word 'mouse'. I do not say that it is a matter of definition, because I do not think that words like 'mouse' are introduced by definitions; but I am sure it is not the result of 'hard work . . . done to establish that the subject-matter under consideration is the kind of thing covered by the putatively relevant propositions' of physics.

(ii) Mr Bennett then turns to my account of the consilience of primary inductions when they are all subsumed under an hypothesis of secondary induction, and he writes :

We allow, then, the claim that L_1 , L_2 , and L_3 (hereafter called the 'L-laws') are more probable than H . But, Kneale continues, consider the situation before H is applied to the L -laws; consider, that is, their relative probabilities in the situation when the evidence for H is what it always was and always will be, but when the evidence for the L -laws is only what it was before H was thought of. At this stage of the proceedings, *H is more probable than the L-laws*. And since nothing happens to the probability of H by its being 'applied' to the L -laws, we must say that something has happened to the probability of the L -laws themselves if we are to account for the fact that they begin with a probability smaller than that of H and end, after being 'explained' by H , with a probability greater than that of H The mistake lies in the italicised clause in the above paragraph. . . . If H logically implies the L -laws, then it is not more probable than they, and this fact cannot be avoided by talking about the 'application' of H to the L -laws. . . . All this is not to deny the doctrine of the consilience of inductions: there clearly is a sense in which particular statements of any science gain in probability when they are incorporated in *some* kinds of wider theory; but only some, for a conjunction of the particular statements together with, say, 'Grass is green' is a 'wider theory' in some sense of that phrase; certainly in a sense adequate for the whole of Kneale's argument.

From the wording of this passage the reader might conclude that he had before him a summary of my argument, and in particular that I had talked about the 'application' of explanatory hypotheses to laws. In fact I have never used this phrase, and I am not sure that I understand what it means in Mr Bennett's account. If, however, it means, as I am inclined to think, the deduction of the laws from the hypotheses, then the clause which Mr Bennett italicises in his account does not express anything I have asserted. So far from thinking that H is more probable than the L -laws before any of these have been deduced from it, I think that at this stage it is a mere speculation. Furthermore, I cannot see what reason Mr Bennett has for talking about a comparison of probabilities in the situation when 'the evidence for H is what it always was and always will be' and for saying that 'nothing happens to the probability of H by its being "applied" to the L -laws'. Transcendent hypotheses are acceptable only when they are empirically confirmed, and they cannot be empirically confirmed unless we are able to derive from them some generalisations about observables. Nor do I say anywhere that after they have been explained the L -laws have a probability greater than that of H . On the contrary, I try to show that the increase of probability which each law gets with explanation is derivative from the theory in which it has been incorporated. Finally, I say

A REPLY TO MR BENNETT

explicitly on page 109 of my book : ' If an hypothesis H were equivalent to a mere conjunction of the laws L_1 , L_2 , and L_3 which it was supposed to explain, there would be no consilience of inductions and the evidence for L_1 would not help in any way to confirm L_2 and L_3 . ' For it is my belief (expressed in various places with a good deal of emphasis) that there can be no explanation worth the name without simplification.

Since I cannot recognise my own views in Mr Bennett's account of them, I do not think I need say any more about his criticisms under this head. But I may perhaps add that I think some of his difficulties in understanding the theory of consilience may be due to his failure to distinguish between probability in matters of chance and the probability (or acceptability) of the results of induction.

2 The Range Theory of Probability

(i) In attempting to develop a tenable version of the range theory of probability I argued that each member of a set of alternatives under a given character might properly be described as equipossible with each of the others if none of them had any sub-alternatives except such as were constituted by conjunction with characters independent of all alike. Mr Bennett does not deny that such S -alternatives, as he calls them, would be equipossible ; but he objects that my theory is probably unworkable because ' S -alternatives just are not specifiable in practice and are not specifiable even in theory except as infinitely complex, ultimate alternatives—individual concepts, in fact '. In short, we cannot in this way ' reach that finitude and definiteness which were the whole aim of the introduction of the theory in the case of open classes '.

Since I do not wish to offer a theory which allows for the estimation of all probabilities *a priori*, I am not worried by the suggestion that S -alternatives cannot be specified in practice. But I agree that my theory would break down if it could be shown that the smallest set of S -alternatives under a given character was an *untameable* infinity (i.e. an infinity that could not in principle be parcelled into a finite number of equal subsets), and I think this is what Mr Bennett wants to show by an involved argument in which he starts from the assertion that ' we must interpret S -alternatives as being sets of characteristics such that none of them has causal consequences not shared by all the others '.

If it were shown to be impossible in my theory for any one of a set of equipossible alternatives to have a consequence not shared by all the others, I should abandon the theory without waiting to hear of the curious conclusions Mr Bennett tries to draw from his assertion. For the whole point of talking about the equipossible alternatives under a character is to prepare the way for saying that a certain proportion of them necessitate some other character while the rest exclude it. But Mr Bennett's assertion about S -alternatives is just a mistake. He has assumed incorrectly that, if everything which is independent of α_1 -ness (i.e. which does not necessitate it and is neither necessitated nor excluded by it) is independent also of α_2 -ness and vice versa, then everything which is necessitated by α_1 -ness is necessitated by α_2 -ness and vice-versa.

(ii) Next Mr Bennett argues that even if the range theory survives his first criticism it is still inadequate as an ' explicans of anything like the concept of probability as

it is ordinarily understood' because there is no satisfactory connection between it and the frequencies which interest men when they talk of probability. 'What warrant', he asks, 'have we for assuming that frequencies are in any way a guide to probabilities in Kneale's sense?' None, he answers, unless we can assume 'that the members of any given open class are distributed approximately evenly throughout all the equal alternatives'. But 'we just do not have the requisite information about how the universe was poured into its original possibility-moulds'. And in any case to say that something about *S*-alternatives 'is what really concerns us when we talk about "probability" is just to show that one has not been listening'.

I am not much moved by the last remark. As I said in my book (pp. 180-1), the range theory is only an elaboration of the talk about chances which has been common among gamblers and theorists alike for centuries. What concerns the gambler (in the Quaker sense of 'concern') is certainly not a distribution of equipossible alternatives. But neither is it the frequency of some attribute in a class of unspecified size. He wants to win money on a particular occasion, and he talks about chances because he thinks they are relevant to his problem of rational planning, as indeed they are.

For the rest, I certainly do not think myself entitled to assert that 'the members of an open class are distributed approximately evenly throughout all the equal alternatives', or indeed anything else about the filling of what Mr Bennett calls the possibility-moulds, except that some are undoubtedly occupied. On the contrary, I have argued in a paper called 'Natural Laws and Contrary-to-fact Conditionals'¹ that the regularity theory of natural law is wrong precisely because it involves the queer thesis that every natural possibility must be realised somewhere at some time. What I have to say about the use of recorded frequencies as a guide in the estimation of chances can be found in §§ 43 and 44 of my book, and so far as I can see it involves no assumption such as Mr Bennett suggests. I suspect that his reason for trying to foist the assumption upon me is simply that he takes some form of the frequency theory for granted and therefore thinks the only way of making my theory at all plausible is to convert it into a frequency theory by an *ad hoc* assumption. But I have tried to show that *there is no frequency theory which makes sense* in relation to open classes, and Mr Bennett himself allows that I have 'good ground for attacking some formulations'. What, then, is the new version he has to offer? Surely one who admires Lord Keynes as he does cannot wish to join in spreading the fallacy that it is *rational by definition* to argue in the form: 'A proportion p of all the α things observed have turned out to be β ; so we should expect about the same proportion p of all the α things observed in the next day (week, month, year, century) to be β '?

3 *Eliminative Induction*

(i) Under this heading Mr Bennett first charges me with making an unjust criticism of Keynes's argument; and here, I am sorry to say, he is quite right. I was in error when I said on page 209 of my book that the sort of eliminative reasoning which Keynes favoured would be useful only in a search for reciprocal connections.

¹ William Kneale, *Analysis*, 1950, 10, 121; reprinted in *Philosophy and Analysis*, ed. M. Macdonald, Oxford, 1954, p. 226.

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Keynes's talk of analogy still seems puzzling to me, but I have no doubt that the second half of the paragraph in which I discuss it should be deleted as a silly mistake.

(ii) Next Mr Bennett argues, not only against me, but also against Keynes himself, that it is possible to describe a situation in which the method of elimination would work even if there were an infinity of possible alternative hypotheses. We should have such a situation, he says, if a single experiment eliminated an infinity of hypotheses at one blow ; for then the probability of our generalisation ' *might* be genuinely increased and might be in a position to go on being increased by the further elimination of merely finite numbers of fresh hypotheses '. I agree with this remark, but I think that it amounts only to the suggestion of a new complication in the Principle of Independent Variety. Instead of saying that the method of eliminative induction will not work unless the set of possible hypotheses with which we start is finite, we now say that the method will not work unless there is some finite number of tests such that the set of possible hypotheses surviving after that number of tests is finite. But it seems clear that Mr Bennett does not intend us to take his suggestion very seriously ; so I will say no more about it.

(iii) More important (at least from my point of view) is Mr Bennett's attack on my objection to all attempts to justify induction within the theory of chances. In this connection he writes :

We shall here pass over Kneale's identification of the theory that there are physical laws which are not just conjunctions (which theory he claims to be essential to the theory of eliminative induction) with the quite different theory that these laws are all of a logical nature. . . . For there is, as Kneale recognises, such a thing as *possibility-on-the-evidence*. And whatever theory of chances one may construct such that ' we are not justified in [talking of] second-order chances ' (p. 213) based on ' second-order possibility ' (i.e. possibility on the evidence), it just is the case that if we start in a state of knowledge in which any one of 100 statable hypotheses might, for all we know, be the explanation of certain phenomena ; and if we move from this position to a state of knowledge in which there are only thirty hypotheses any one of which could, for all we know, be the required explanation ; then we have raised the likelihood of each one of the thirty hypotheses. It will be readily seen, in fact, that what we have here is just a special case of the frequency theory of probability. . . . Nor is it true, as Kneale affirms, that ' the project of dealing with induction in this way has been encouraged by the indifference theory which makes ignorance a sufficient ground for assertions of probability ' (p. 213).

And in the final paragraph of his article he adds :

. . . there is no reason why the probability, *relative to given evidence*, of a scientific hypothesis should not be expressed in a fraction. This seems impossible not because of some impropriety in the whole idea but simply because we never know what value the fraction has.

These passages require a number of comments.

In the first place, I have not said that physical laws are ' all of a logical nature '. What I have asserted is the quite different thesis that the notion of necessity used in

natural science is the same as that used in logic and phenomenology, namely, the notion of a situation without alternatives. This remark is intended, of course, to be a repudiation of recent philosophical patter about 'logical necessity' and 'causal necessity', but it does not imply that I think laws of nature are formal truths like the theses of Aristotle's *Prior Analytics* and Frege's *Begriffsschrift*, nor yet that I think they can be known *a priori*. If, as Mr Bennett says, most modern philosophers find it strange, the reason may be that they have accepted too readily the doctrine that the necessity of truths known *a priori* (including, of course, those of logic) consists in their being 'made true by definition'. For it is an obvious consequence of this doctrine that truths which have been established empirically cannot be necessary in the same sense as truths ascertainable *a priori*. For my own part, I do not think that any truths are made true by definitions, unless indeed we are to say that Dr Zamenhof made an infinity of new truths when he invented Esperanto.

Secondly, what Mr Bennett here calls possibility-on-the-evidence should be called rather possibility-on-the-lack-of-evidence. For it is not the known compatibility of something with all our data, but rather the absence of knowledge sufficient to refute a suggestion. In short, it is just such possibility as we express when we say 'Goldbach's conjecture *may* be true'. If anyone thinks that because a number of hypotheses of connection between characters are possible in this sense he can talk sensibly of the chances of truth for any one of them, he is committed to the alchemy of distilling knowledge from ignorance.

Thirdly, Mr Bennett seems to be altogether too easily satisfied when he says that, if we start with a hundred hypotheses 'any one of which might, for all we know, be the explanation of a certain phenomenon' and presently reduce our list to thirty by elimination, we have increased the likelihood of each of the remaining hypotheses. Since there is nothing in his statement of conditions to indicate that the hundred hypotheses with which we start are all the hypotheses we do not know to be false, they may, it seems, be just a selection from an infinite set. But if it makes sense to speak at all of chances here (which I deny), the chance of correctness for each of the hypotheses on our list may then be infinitesimal, not only at the beginning, but even after we have eliminated seventy. Furthermore, even if the original list of a hundred hypotheses were known to be exhaustive, the elimination of seventy might do nothing to raise the probability of any of the rest. For it is still conceivable that the seventy deleted all had infinitesimal probabilities at the beginning. How, if at all, does Mr Bennett exclude this inconvenient suggestion? He makes the surprising statement that his argument is a special application of the frequency theory of probability. This seems to me to be a strange misuse of terms by which he hides from himself that he is relying on the principle of indifference to justify an assumption of equal probability for all the hypotheses surviving in his list at any time. It was precisely such faulty reasoning that I wished to exclude when I said that we could not talk properly of chances in this connection.

Fourthly, even if Mr Bennett could defend his account of induction against the criticisms of the previous paragraph, he would still be committed to the strange conclusion that in the very best induction the probability of an hypothesis went up through the sequence of values . . . , $\frac{1}{4}$, $\frac{1}{2}$, $\frac{1}{2}$, 1, without ever taking a value in the interval from $\frac{1}{2}$ to 1, i.e. that no hypothesis ever became probable at all in the ordinary sense of that word. He says that the probability of a scientific generalisation can be

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represented, in principle at least, by a fraction, but I do not think he can really wish to defend a theory which involves this paradoxical consequence.

(iv) In his last sub-section Mr Bennett objects to my saying on page 213 of my book :

When . . . we try to work out what is involved in talking of the chances of there being a certain probability relation between two characters, we must first think of the characters as constituting an ordered pair and then suppose there is some initial probability of this dyad's exemplifying a certain probability relationship *simply because it is a dyad of characters*.

First he says that what concerns us is the probability of a *causal* relationship between characters, and next he denies that we need to talk of the chances of there being such a relation between two characters merely because they form a dyad.

My reason for talking here about the chances of the holding of a probability relation between characters was that I wished to discuss not only the establishment of laws but also the establishment of probability rules (in my sense of that phrase) and had already noticed that the first might be considered as a special case of the second. And my reason for talking of the chances of the holding of a probability relation between two characters merely because they formed a dyad was that I wished to draw attention to the queerness of the assumption of initial chances required by the arguments I had set out in my two previous sections. Mr Bennett ignores the context of the passage which he quotes with disapproval, and assumes incorrectly that my discussion was concerned only with arguments to establish laws.

WILLIAM KNEALE

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POPULATION STUDIES AND SCIENTIFIC METHODOLOGY

THREE recent books on animal ecology¹ show how hard it is to create a science out of data which are still preponderantly descriptive. Any science in its early years is said to be at the natural history stage, or only by courtesy to be a science at all, and the term 'natural history' often has derogatory implications. Animal ecology, it is true, has so far been largely concerned with natural history and little with verification through theoretical and experimental science; but this preoccupation has probably been necessary and desirable, since the creation of a new discipline is helped by a certain amount of isolation from orthodox points of view, and premature systematisation is more likely to hinder discovery than advance it. Nevertheless, as the present article may indicate, ecological thought might benefit from a closer link with established forms of scientific methodology.

Towards the beginning of its modern development the theory of animal populations was profoundly influenced by the Lotka-Volterra models, many of which have been put together in a most useful book by D'Ancona. These models demonstrate, for example, that two species will fluctuate in numbers, or fail to co-exist, if they influence one another in specified ways, even though all physical conditions remain constant. These conclusions, being implicit in the premisses, cannot, of course, be falsified; but through no fault of the mathematicians they have been applied to nature without sufficient regard for important structural dissimilarities between the concrete and abstract systems. The very precision of the models seems to have led to the common error of supposing that their material truth might be assumed, although qualitatively similar results are consistent with many other hypotheses which cannot be eliminated, as in the physical sciences, by appeal to quantitative predictions. The present book gives too little attention to the criteria by which the applied mathematician judges the relevance of his constructions to nature (compare, for example, Kendall's modest attitude towards his own models for much simpler systems²), and unless the rôle of mathematics is made clear to the biologist it seems likely that more harm than good may be done. The importance of the models is that they are consistent with the belief that certain types of population change are related

¹ Umberto D'Ancona, *The Struggle for Existence*, Leiden, 1954, Pp. xi + 274. H. G. Andrewartha and L. C. Birch, *The Distribution and Abundance of Animals*, Chicago and London, 1954, Pp. xv + 782, £5 12s. 6d. David Lack, *The Natural Regulation of Animal Numbers*, Oxford, 1954, pp. viii. + 343, 35s.

² David G. Kendall, *Symp. Soc. Exp. Biol.*, 1953, No. VII, 55

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to biotic processes, and not necessarily to changes in the physical environment. Abstract verifying instances, however, are hardly sufficient to establish the general validity of such propositions, and the frequent confusion of deductive and material truths seems to account, at least in part, for the tendency of ecologists to divide into the opposing schools of thought represented by the writers of the other two books.

There is, then, much to justify the criticisms levelled by Andrewartha and Birch against this widespread misuse of mathematical models and the resultant mental attitudes; but the authors also seem to deny the value of the simple, ideally isolated system as a first step in creating order out of chaos. They object, for example, to the 'unnatural and stereotyped uniformity' of the animals and environments in the models. Such objections are those of the traditional empiricist, who is unwilling to argue from assumptions which, though unrealistic, can be used as units of description. The mathematician's job is to create structural relations; their physical interpretation cannot be found by purely intellectual means.¹ It is irrelevant to an argument that a relation is described as, say, 'competition for food', since the same consequences follow if it be assumed that the animals compete for trace elements or merely influence one another in some biologically unspecified manner. The mathematician, if he wishes to interest the biologist, must usually attach a biological meaning to his terms; but it is not necessarily correct to reject his mathematical structure on the grounds that it cannot be properly visualised in concrete terms. Andrewartha and Birch, in denying the value of a well known model, claim to have exposed a contradiction, which arises, however, only when a biological meaning is attached to a relation which should be expressed in completely general mathematical terms.

Lack also cautions his readers against the misuse of mathematics, but does not make it clear that similar caution must be used in the application of any deductive argument. Only three factors, according to Lack, can regulate natural populations (disease, predators or parasites, and food shortage); numbers cannot, as a rule, be regulated by the action of the physical factors. This distinction seems to arise partly because the physical factors are confused with their effects on the organism, partly because the search for regulating factors is restricted to the type most likely to be both necessary and sufficient. Lack recognises, however, that a given factor does not always satisfy both conditions. The majority of Lack's arguments, closely knit and eminently reasonable, follow from similar premisses, which he does not re-examine, even when the evidence, certainly that for clutch size in tits and ducks, suggests that this course would be prudent. Although he presents much contrary evidence, he treats it in a way that illustrates, perhaps too thoroughly, the falsity of the view that even a single phenomenon is enough to disprove a hypothesis.

¹ C. A. Coulson, *The Spirit of Applied Mathematics*, Oxford, 1953, p. 23

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In contrast to Lack's essentially scholastic outlook, the pure empiricist is apt to be satisfied with the 'obvious and simple' explanation that the physical factors are sufficient to regulate abundance. At times Andrewartha and Birch appear to belong to this school of thought, their beliefs being based on the frequent association between changes in weather and population density (though these associations are mostly *a posteriori* descriptions, unverified by prediction). At other times these authors seem to have unified the rival points of view. The action of the physical factors, they claim, cannot be understood without regard to that component of the environment which they term 'other animals of the same sort'; and by defining the environment of a population as the sum of the individual environments they attempt to remove the current dichotomy of view that distribution, but not abundance, may be determined by the physical factors (or certain types of mutation).

The most valuable contribution made by these authors is perhaps their integration of experimental and field ecology. There are plenty of hypotheses in ecology; but the traditional way of verifying a hypothesis, by sampling from its logical consequences, is unacceptable to certain naturalists because of their mistrust or misunderstanding of experimental methods. Thus, on the grounds that the animals are kept under artificial conditions, Lack feels justified in rejecting all but a few selected results from the work on experimental populations. (It is fortunate that Torricelli did not abhor a vacuum and that Darwin was willing to learn from variation under domestication.)

Lack finds no evolutionary unity among the processes determining abundance, even among the closely related game birds, but has sought his order within another doctrine ('The function of natural selection is to select—not to produce balance'¹). On the other hand, his view that clutch size has been adapted by natural selection to the amount of available food, seems to introduce diversity rather than unity into the interpretation of birth-rates and death-rates in general; for his system can be preserved only by making numerous *ad hoc* explanations, which destroy the generality which should characterise a hypothesis. Andrewartha and Birch, using the less simple postulates of modern demography, have introduced greater systematic simplicity, while D'Ancona, being least hampered by material issues, presents the most unified scheme of all.

Although sampling from a common pool of knowledge the authors present three different, though rational, points of view. The difficulty for the reader will be to judge the truth until hypothesis is more frequently disciplined by experiment, and proportionately less by faith and reason. No one should dispute, however, that each book makes a valuable contribution towards the solution of some difficult problems.

DENNIS CHITTY

¹ A. J. Nicholson, *J. Anim. Ecol.*, 1933, 2, 132

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Physics, Psychology and Medicine: A Methodological Essay. By J. H. Woodger.
Cambridge University Press, London, 1956. Pp. x + 146. 8s. 6d.

THIS book serves two distinct purposes, both equally well. It is primarily addressed to medical students and has the aim of helping them to cope effectively with the clinical problems presented by sick persons. But it makes, at the same time, a contribution to philosophy that is by no means negligible. The author has, in particular, much to say that is both significant and original about the nature of reality.

The two purposes assist each other. The philosophy helps the medical student to arrive at the right kind of generalisations; the need, on the other hand, for being comprehensible to students who have not had a specialised training in philosophy has been an incentive to clear exposition. The author's effort to give the medical practitioner more insight into psychology than is provided by conventional medical studies is justified by a statement given to the House of Commons in 1954 and quoted in this book that out of 500,000 hospital beds under the National Health Service no fewer than 211,000 were for mental and mentally deficient cases. Nevertheless, as Professor Woodger says, the student is taught to regard human beings as no more than complicated pieces of machinery whose parts can go wrong in various ways. 'But,' he adds, 'being miserable is not a way a machine can go wrong. Its correction seems to call for a different approach.' And later: 'We thus have two states of affairs: a medical training which is overwhelmingly physical, and a sick population containing a high proportion of cases which are not classified as physical.' To help these far more is obviously needed than a good bedside manner and a soothing personality. The medical practitioner also needs insight. When a patient has failed to come to terms with his emotions it is 'the duty of the physician to help him to discover the source of his conflict and to overcome it'.

It is pointed out that the present excessively physical bias in the medical training arises from the very great prestige that the physical sciences have achieved in recent times. The aim of physics is complete objectivity; subjective statements have no place in its discipline. The only valid statements in the physicist's universe of discourse are impersonal ones, statements that eliminate the observer. As Professor Woodger puts it: 'In the early days of physics it was necessary to exclude the notion of person from science because in the province of physics it was not applicable. The resulting success of the physical sciences has not unnaturally had the effect . . . of excluding this notion from science permanently, even from the science of persons itself.' Would that sundry prevalent schools of psychology could be persuaded to take note of this situation and reconsider their belief that psychology can be scientific only when all psychological contents have been

eliminated from it. In these schools anthropomorphic statements are deprecated, even about man ; but not, most oddly, about electronic calculating machines !

While Professor Woodger does not, of course, deny the overriding importance of physiological study in a medical training, nor that this is principally a study of physical and chemical processes, he says sensibly ' it would seem that those who wish to emphasise the machine-like features of men would be well advised to content themselves with saying that men are machine-like, rather than that they are machines. Then there will be nothing to dispute about and time and effort can be spent more profitably. '

It will already have become apparent that Professor Woodger attributes the present somewhat lopsided system of medical training to a defective philosophy. As a defective philosophy is, in turn, the consequence of an inadequate methodology, some space is given to logic and scientific method. A chapter entitled ' The Status of Explanatory Hypotheses ' contains in seven pages as much substance as would have occupied over three times that space if couched in conventional philosophical language. Yet there is no lack of precision—a considerable achievement. A point that is stressed here and has a particular bearing on the philosophy of science concerns explanatory hypotheses. It is, says Woodger, ' erroneous and most misleading to speak of *verifying* an explanatory hypothesis. However many times observations may confirm the predictions of such a hypothesis, this does not entitle us to say that the hypothesis is true ; although when a hypothesis is successful it is difficult not to believe that it is true. ' This is obvious and has been said in a variety of ways before. But it has not yet been said often enough to prevent people over and over again from claiming in the name of science that they have verified a statement when it is no more than an unproved and unprovable hypothesis. The claim arises from the regrettable notion that all hypotheses are better ignored than faced, as though they were scientifically disreputable. There would be less temptation to ignore them if the difference between verifying and justifying a statement were better appreciated. One can *verify* a tautology but one can only *justify* a hypothesis, and this only in one way, namely by demonstrating its explanatory power. The book under review is likely to help the student to understand this. When he learns what a lot of hypotheses he uses every day he will be less inclined to insist that all traditional beliefs are irrefutable fact and to dismiss all new ideas as ' mere hypotheses '.

The defective philosophy that Professor Woodger is seeking to correct is the one that denies, be it explicitly or by implication, the reality of subjective experience, that ignores the difference between the active subject and the passive object, and that sees the universe as composed exclusively of interacting material systems. The reality of subjective experience and the need for making a clear distinction between a person, his experience, and the

system on which he acts is presented in two chapters with the respective titles 'Getting' and 'Doing'. The approach is unconventional and none the worse for that. 'Tom is getting a view of the sea from his bedroom window' is given as an example of a statement in which information is conveyed about an observer, an object that is observed and the subjective experience of observing. Those philosophers who claim that the physicist's world of objective reality is the only real one should, if they were consistent, deprecate the form of words used in the above sentence with its implications of an irreversible relation between subject and object and of a subjective experience that has no place in the physicist's universe of discourse. This means that one can preserve the philosophy according to which completely objective statements are the only true ones, but only at the cost of stripping one's speech of many statements with a meaningful content. This unconventional way of exposing the fallacies in a whole collection of 'isms' is neater than the method of ponderous analysis to be found in many essays with a more imposing appearance. This part of the book is pivotal to the whole argument and is, in the present reviewer's opinion, too condensed. Points that are made in a few words would better have been expanded into a paragraph. The chapter on 'Getting', in particular, repays being read twice. If Woodger were here merely reproducing one of the accepted and fashionable philosophies, the unconventional and over-condensed approach would not matter; one could safely ignore what he says. But what is said here runs counter to the prevailing fashion and deserves for this reason, if for no other, to be noted carefully.

Another way of expressing the defective philosophy from which the author would save the medical student is concisely put in the words 'another tenet of this philosophy seems to be that everything that is, is in a big box called space, which is floating down a river called time. Consequently, if anything (except the river !) is not in space, it is just not at all.' Professor Woodger calls this 'the finger and thumb philosophy' and thereby conveys, I fear, the erroneous impression that this philosophy is an oddity subscribed to only by an eccentric few. But in fact it is at the root of many of the fashionable 'isms'. The notion that the big box called space is the container of all active reality is hard to get away from. According to what is probably the majority view of reality any activity in the big box must necessarily originate with something else that is also in the box, and thus have location, be capable of detection by physical means and be described as a constituent of the material universe. The big box philosophy attributes all events, be they subjective or objective, to the unaided action of matter on matter. One philosophical system after another has been developed in a frantic attempt to preserve this notion. An early attempt was called 'physico-psychological parallelism', and another the theory that mind is an epiphenomenon. Somewhat later the philosophy of emergence came into fashion in which it

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was claimed that the *relationship* between various contents of the box was the active cause of everything that can be observed, including subjective experience. A present attempt to preserve the notion is by the construction of mechanical models of the human brain or of a tortoise. It is claimed that, with sufficient elaboration, models constructed to such patterns would truly represent the mind-body relationship. Every component part of these models is material and thus has its location in the big box. Those who claim that a sufficiently elaborate model would represent the whole system deny the existence of influences without location. What cannot be found in the big box is then said to be no more than a 'ghost in the machine', incapable of influencing the way the contents in the box act on each other.

In this connection another passage in the book under review is illuminating. It is concerned with metaphors used in psychology. Woodger points out that these employ a language applicable to objects with location, in other words to objects within the big box. He points out that such metaphors do not mislead so long as one does not take them literally. In metaphor, he points out, 'we say what we do not mean in order to convey what we do mean'. Nevertheless, one does tend to take the metaphors too literally; one does so, for instance, when one speaks of a notion as being 'inside' one's head. Such habits of speech only confirm the philosophy according to which the mind-body problem can be wholly solved by a study of the contents of the big box. One might get away from the wasted time and effort spent on the mechanical models if the expression 'mind-body relationship' were to be replaced by 'mind-space relationship'.

As it is, the big box philosophy has enslaved not only the cruder materialists but also some of the more idealistic philosophers. These often get no further than to assert that the big box, as it floats down the river of time, contains, in addition to gross matter, many fine things, thoughts and feelings, love and beauty, each properly located in space. It is difficult indeed to surrender the notion that what is is somewhere. It is impossible to predict what next attempt at preserving the big box philosophy will become fashionable, but let it be hoped that Professor Woodger's book will have enough influence on future medical practitioners and others to make it a little more difficult for the next attempt to appear plausible.

R. O. KAPP

The Unified System Concept of Nature. By Stephen Thyssen Bornemisza. Vantage Press, New York, 1955. Pp. viii + 131. \$3

THE enterprising though rather laboriously worded title of Dr Bornemisza's book is a fair indication of what follows: its aim is nothing less than the establishment of the fundamental principle, or principles, of nature, of the

forces that hold the universe together, and of the origin and character of life itself. But, as in the title, the style is heavy and cumbersome, and often extremely unclear. Unfortunately, as one finds out later on, not all of the unclarity is due to clumsy expression, or merely to a loose way of talking, but to a considerable disregard for logical consistency. The book contains several contradictory or mutually exclusive definitions of important concepts (one pair of them on the same page in immediate succession (p. 35), and, more often than not, controversial statements about the connections between some matters of fact, which the author cites to bear out his theory, are made without any attempt at justification or proof.

Nevertheless, Dr Bornemisza has several very interesting and important things to say, and he says them forcefully and confidently.

His view of the world is frankly dualistic. He believes nature to be exhaustively divided into two types of activity which he calls the 'recurrent' and the 'structural changes'. By 'recurrent changes' he means all types of cyclic processes, including waves and oscillations, spin and orbit of electrons and planets, circulation of water on earth, metabolism, etc. He calls these processes which occur in a limited space, 'self-maintaining systems' which operate on a feed-back principle, but involve a small additional stimulus from outside the system (i.e. the systems are not closed, and their 'boundaries are transgressible and fictitious'). He clearly distinguishes them from the reversible processes in the thermodynamic sense. These 'self-maintaining systems represent all those structures that are carriers or vehicles of recurrent changes, i.e. of *circular causation*' (my italics). Each self-maintaining system is divided into other sub-systems of the same type, in a hierarchical order.

The 'structural changes' are those that take place unsystematically, by chance, and leave the state of the universe permanently altered; obvious examples are biological mutation and radio-active decay, de-radiation of stars, and growth and death of an organism. He makes the interesting point that chance acts so as to prevent the universe from falling to pieces; it is a way of correcting mistakes and disturbances by switching away from chains of cause and effect leading perhaps to unavoidable disaster' (p. 4). He considers it not surprising that there should be randomness in nature, 'but that an old condition long since past should, from the infinite abundance of all possibilities, be selected anew in order to be reproduced once more, is the most astounding fact that nature discloses' (p. 8). This makes it look, he says, as if 'nature is endowed with a memory, . . . which leads over and over again back into the past' (p. 18).

Unfortunately, having described these two useful concepts, he extends them to cover the social as well as the mental sciences, and clearly wants them to do too much. (The daily output of a factory, traffic in a street, visitors to a museum are given as examples of 'recurrent changes' in the

social field, and the founding of a business, growth of a town, and the outbreak of war cited as 'structural changes'.)

At the end of Part I he states that all recurrent changes are not completely realisable, but combine eventually to form a structural change—if we consider a long enough period of time. The duality is thus resolved to allow for only one type of phenomenon: 'The universe is inhabited by nothing but self-maintaining systems' (p. 24) which ensure the greatest value of the entropy integral, i.e. 'the utmost duration of the utmost degradation of energy' (p. 38).

His second main theme, developed in Part II, is what he calls the 'fitness' or 'purposiveness' of organic activities, i.e. the adaptation of the organism to its environment. Although the author phrases this contention carefully ('the organisms *create the impression that they follow* a design directed toward the distant future', p. 58, my italics), one feels that his approach is definitely teleological, although he denies this in one sentence towards the end of the book. To explain the apparent purposiveness of organisms, he introduces the 'Concept of the Organic Image' which is a reproduction or representation of the environment in the organism. From the properties he attributes to it, we would call his 'image' a 'model', since he says that it reproduces the environment only relatively and relationally, and we would suspect it to be something like the imperfect, Platonic manifestation of the original ideal.

By 'environment' he means the temporal as well as the spatial surroundings, and thereby attempts to explain heredity, for instance, as traces, i.e. structural changes, left by past experiences on the cells and the chromosomes, which now develop to reproduce the environment that gave rise to them. Unfortunately, he then turns round and postulates a 'physical substratum' for the organic image, and states that the brain is the seat of the 'central image'.

From here on, his development of the concept of the organic image is entirely materialistic, even mechanistic: the environment is reproduced in the brain by means of the sensory organs and nerves, but, reciprocally, our thought and volition influence the environment through the afferent nerves, and muscles, and by motoric actions. Language is an extension of this movement. He identifies this mutual interaction with active *v.* passive, internal *v.* external tendencies of the organism, which again bears out his dualistic view of the forces in nature.

The combining link between the two parts of his book is the use he makes of entropy which he shows to be involved in the structural changes on the one hand (it is only they that have time direction), while it ensures the longest possible continuation of life on the other, since organisms act so as to increase the entropy of the universe. Even our psychological behaviour is ultimately aimed at the dissipation of energy, and it is those events which

demonstrate a degradation of energy that produce the sensation of pleasure in us. (We enjoy watching a waterfall, a fire, the escape of gas from a balloon, etc.)

The fourth, and last, point to be mentioned is his contention that life did not originate from inorganic matter, but has always existed, though perhaps in a different form. He believes that we are unjustified in limiting life to carbon molecules, but that we should extend the concept to atoms as well. We have no right to assume, he says, that just because we are unable to observe sub-atomic action, life should cease at the border of our observability. 'Atoms and molecules are representing "organisms" on another level of evolution' (p. 97), and life is 'the active faculty of the self-maintaining system to reproduce the space-time environment in the present' (p. 114). And he defines the *origin* of life as 'an immanent property of . . . the atoms and molecules to be introduced into super-ordinated systems' (p. 116).

What he means by 'an immanent property of the atoms', or by 'another level of evolution', we can only guess. On the whole, the reader will have to provide a fair amount of guess-work, and an even larger portion of patience and good-will, if he wants to benefit from Dr Bornemisza's essay, for sentences like 'Atoms are "born", for they exist; they "die" of old age, and can be "killed"' (p. 97) are hard to forgive. The appendix, which is called 'A Systematization of Psycho-Physical Phenomena' and ends with a side-by-side exhibition of the psychological and physical terms introduced or used in the book, should be omitted by all readers, since it re-defines standard philosophical terms such as 'sensation', 'perception', and 'cognition' in an entirely new and confusing way, making it quite clear that the author, well-known geophysicist as he may be, has either little acquaintance with, or no regard for, established philosophical terminology, nor for methodological practices.

Apart from one or two obvious misprints, and a few Germanisms (such as using 'theory of cognition' instead of 'Theory of Knowledge' for 'Erkenntnistheorie'), there is only one error worth calling attention to: On page 77, it should read 'if $p_1 > p_2$ and $s_1 < s_2$ ' (instead of ' $s_1 > s_2$ ').

EVA CASSIRER

The Interpretation of Nature and Psyche. By C. G. Jung and W. Pauli.
Routledge and Kegan Paul, London, 1955. Pp. 247. 16s.

THIS work was first published in 1952 by the C. G. Jung Institute in Zurich under the title *Naturerklärung und Psyche*. It contains two long essays, Professor Jung's entitled 'Synchronicity: an Acausal Connecting Principle',

Professor Pauli's 'The Influence of Archetypal Ideas on the Scientific Theories of Kepler'. Jung's essay, the longer and more ambitious of the two, has been revised for the English edition.

Jung offers us this essay as a tentative exploration of 'a very obscure field'. It is not easy reading. The style is discursive and the arguments and definitions not always consistent. Jung's concern is with the problem of explaining extra-sensory perception and related phenomena, of which he claims to have found many instances in his clinical experience. *Prima facie* we are obliged to choose between two alternatives—dismissing such facts as chance coincidences, or assuming a causal connection between the events in question. Jung argues that both alternatives must be rejected: that the chance-theory is ruled out by the significant scores obtained in card-guessing experiments by Rhine, Soal, and others; and that, in view of the evidence that ESP functions independently of distance and sometimes takes the form of precognition, 'it is impossible . . . to explain ESP . . . as a phenomenon of energy. This makes an end of causal explanations as well, for "effect" cannot be understood as anything except a phenomenon of energy' (p. 27). Jung offers us as a third alternative the Principle of Synchronicity. He gives a number of different definitions of 'synchronicity' (pp. 36, 40, 44, 144). Collation of these seems to indicate a definition on these lines: the concurrence (not necessarily simultaneously) of a 'psychic state' and one or more 'external events' which are 'meaningfully but not causally connected' with each other. 'Synchronicity' is intended not simply as a new label but as an explanatory principle. Jung speaks of the principle of Synchronicity as being complementary to the principle of Causality. His thesis is that the world contains two irreducibly different kinds of order: 'that events in general are related to one another on the one hand as causal chains, and on the other hand by a kind of *meaningful cross-connection*' (p. 16). The co-existences of these two types of order would, as Jung acknowledges, seem to involve something like Leibniz's pre-established harmony. There is no explanation in the earlier chapters of the sense in which 'meaning(ful)' is being used. It is not until page 91 that Jung reveals what is in his mind: 'the hypothesis that one and the same (transcendental) meaning [i.e. archetype] might manifest itself simultaneously in the human psyche and in the arrangement of an external and independent event'. He accepts this hypothesis as the only alternative to regressing to the notion of 'magical causality'. It is not easy to see why Jung should think it helpful to invoke the archetypes. He admits the difficulty in connecting archetypes with card-guessing experiments (p. 34). He emphasises that we must not think of archetypes as *cause-factors*. Though we may say that an archetype 'underlies' a psycho-physical correspondence, 'underlies' in this context 'does not refer to anything causal, but simply to an existing quality, an irreducible contingency' (p. 138).

It looks as if Jung's explanation of ESP is after all a chance-theory, but with the chance-factor involved at a new and unfamiliar level.

This essay could scarcely have been written, at any rate in its present form, if Jung had paid more critical attention to the concept of causation. Jung assumes that the transmission of physical energy is an essential element in the concept of causation, and it is because it seems impossible to apply this type of explanation to ESP that he decides that the latter must be described as 'acausal'. Jung does not distinguish the two stages in finding scientific explanations: (i) establishing empirical generalisations, correlations between variables that are controllable and/or observable; (ii) theory construction, involving the postulation of unobservable factors in order to co-ordinate a set of empirical generalisations. It is surely a *non sequitur* to infer that ESP phenomena are acausal on the ground that the unobservables currently postulated by physics (e.g. electro-magnetic waves) are not applicable thereto. Empirical generalisations may properly be called 'causal laws' in advance of finding a theory to co-ordinate them. Nor has psychical research failed completely to establish causal laws in this sense, though admittedly what has so far been advanced scarcely provides an adequate basis for theory-construction. The danger of Jung's theory (terminology) is that, taken literally, it would discourage investigators from looking for causal laws, would imply that the phenomena must be accepted with pious resignation as unexplainable, in the sense in which science is concerned with explanation, and that the only kind of explanation obtainable would be identification of the relevant archetypes by someone versed in this art.

Pauli's essay, clearly and economically written and scholarly in its documentation, is a most interesting contribution to the history of science, and raises, moreover, some fundamental questions concerning the nature of scientific thinking. Pauli's aim is 'to illustrate particular views on the origin and development of concepts and theories of natural science in the light of one historical example' (p. 151). Rejecting a purely empiricist standpoint, he deems it necessary to ask what is the nature of 'the bridge' between sense-perceptions and scientific concepts. Accepting, apparently, Jung's theory of archetypes, he argues that 'as ordering operators and image-formers . . . the archetypes function as the sought for bridge' (p. 153). Kepler's thought is selected for study for two reasons: (i) Kepler's theory about the nature of scientific knowledge was in some respects similar to Pauli's. Kepler held that the aim of science is to discover the precise form of ideas (geometrical harmonies) which exist in God's mind, are embodied in the physical world, and are implanted in the soul in the form of images which Kepler called 'archetypal'; (ii) Kepler's search for the laws of planetary motion and optics was, as Pauli shows in some detail, motivated and guided by a peculiar religious symbolism,

notably his geometrical representation of the doctrine of the Trinity. Despite the animistic and archaic elements in Kepler's cosmology, his controversy with Robert Fludd reveals that Kepler was, in the crucial respect, a scientist: the 'harmonies' deduced from his symbolism had to be tested not only by mathematical demonstration but by observational data (pp. 194-200). The critic may not think it necessary or helpful to bring in Jung's archetypes to explain Kepler's cosmological model, for its sources in the ideas of Pythagoras and Plato, Alchemy and Christianity are obvious enough. Such criticism does not impair the value of this study of Kepler's thought as 'a remarkable intermediate stage between the earlier magical-symbolical and the modern quantitative-mathematical descriptions of nature'

C. W. K. MUNDLE

Science and Christian Belief. By C. A. Coulson.

Oxford University Press, London, 1955. Pp. x + 127. 8s. 6d.

THIS book is based on the John Calvin McNair Lectures delivered at the University of North Carolina, Chapel Hill, in 1954. The ambitious object of these lectures is, in the words of the founder 'to show the mutual bearing of science and theology upon each other, and to prove the existence and attributes, as far as may be, of God from nature'.

Professor Coulson seeks to establish 'the propriety of holding Christian views at all, in an age so profoundly influenced by scientific discovery and scientific thought' and to show that 'both by its actual practice and from the nature of its presuppositions, (science is) none other than a religious activity'. He has previously published the main lines of his argument and much of it is, perhaps, beyond the scope of this *Journal*. I shall content myself with some comments on the account of scientific method upon which his central argument is based.

Coulson, in seeking to show that science and religion are more alike than they are usually thought to be, makes two fundamental points.

(1) Recent advances in science have led to a radically revised view of scientific method, which stresses, among other things, the influence of the observer on the situation being studied. These advances have shown that 'the things we thought we were describing do not have the properties we thought they had'. Einstein showed us 'that there was no such thing as an absolute position, or an absolute velocity,' and Heisenberg that 'no one person could ever exactly repeat the same experiment, nor could two different people ever make exactly the same measurement'. One reason for this was 'that the act of measurement, whether in psychology or physics, altered the system measured. . . . To ask a question of nature was to affect

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her, to change her, by however little. . . .’ This argument contains one of the most popular contemporary fallacies. There are two connected points.

(a) On what grounds are conclusions about observations under very special conditions, such as high velocities, vast or minute distances, or the conditions holding within the atom, generalised to apply to observations under *all* conditions? How can it follow, for example, from Heisenberg’s Uncertainty Principle alone, ‘that no one person could *ever* exactly repeat the same experiment’? (I am, of course, assuming that we can discount the senses of ‘exactly repeat’ and ‘the same experiment’ which make this statement analytically true.) To argue that because electrons are not like billiard balls, neither are billiard balls, is to make a type distinction and then ignore it.

(b) Coulson’s conclusion that ‘To ask a question of nature was to affect her, to change her, *by however little*’ is misleading if it is meant to be a statement about science rather than part of an idealist metaphysic, since for the great majority of observations this change is irrelevant. Indeed, in most situations it is scientifically meaningless to assert its occurrence because it is so small as to be well within the range of experimental error and so undetectable.

Coulson goes on from this to make some extraordinary statements about scientific laws. A law, he says, ‘is essentially a description of the results of observations. A scientific law does not control events: otherwise we could not alter it at our whim when we were dissatisfied with it. It is a means of correlating experiences’. The two descriptions of a law here seem to be incompatible since to say that it is a means of correlating experiences is to deny that it is essentially a description of the results of observations. Moreover, the suggestion that we can alter a scientific law *at our whim* is surely just false and suggests that a dissatisfaction quite lacking in any rational basis may justify the alteration of a law. But the dissatisfaction which leads us to alter a law must surely spring from its failure to fit the facts (or our experiences), from internal inconsistency or from lack of economy and to alter a law on some such grounds is not to do so at our whim. Coulson’s confusion here seems in part to depend upon a misunderstanding of a statement, quoted with approval from Kant, that ‘Our intellect does not draw its laws from nature, but imposes its laws upon nature’.

(2) The second fundamental point made by Coulson is that the scientist accepts certain beliefs and presuppositions which are unprovable by scientific means. There are, among others, beliefs in the importance of integrity, passion, and humility; the acceptance of certain moral principles; and, most important for our purpose, presuppositions that facts are correlatable and that there is order and constancy in nature. Some of these presuppositions

would perhaps be better regarded as rules which must be obeyed if communication is to be possible in any field, and are not peculiar to science. What chiefly concerns us here is whether the presuppositions which seem to be more peculiarly involved in scientific investigation, such as those concerning order and constancy, help to show that science is a religious activity. These seem to me to differ in an important way from the fundamental unprovable presuppositions of religion. The belief that there is order and constancy in nature may be psychologically necessary in order that men shall go on looking for particular instances of regularity but, so far as I know, no one has shown that it is logically necessary for the understanding of scientific conclusions. But the belief in the existence of God is logically necessary to the seeing of nature as the work of God. No particular conclusion of science needs a statement of the general regularity of nature among its premisses but the Ontological Argument and any attempt to use the world as evidence of God's existence logically require an assertion of God's existence among the premisses. This fact marks an irreducible gap between scientific and religious argument. Coulson says that God reveals himself in scientific work, and everywhere else, *for those with eyes to see*. But to have the eye of faith we must first have faith. I cannot see, on the other hand, how a lack of belief in the general regularity of nature can prevent us seeing, or understanding when we have seen, that at regular intervals it gets dark.

Clearly, as Professor Coulson argues, no logical inconsistency is involved in holding religious beliefs and at the same time accepting the conclusions of science but, equally clearly, he has not shown this, because his argument rests upon an inadequate philosophy of science. It is a pity that when he turns to philosophy he is content with standards of scholarship and logic which would never satisfy him in his science.

PETER ALEXANDER

Expanding Universes. By E. Schrödinger.

Cambridge University Press, London, 1956. Pp. 93. 17s. 6d.

PROFESSOR Schrödinger's new book, *Expanding Universes*, does not claim to be an exhaustive treatment of this subject. It is based on a brief course of lectures originally delivered to an advanced seminar group, and it concentrates particularly on the de Sitter theory of the universe. This particular theory allows of several equally simple geometric representations. Next, the book deals with the theory of geodesics, and finally with waves in general Riemannian space-time and in an expanding universe.

As a model of the de Sitter universe, Professor Schrödinger considers a 4-dimensional hyper-surface, which, to be made visualisable, is reduced to an ordinary (i.e. defined by x, y, z) one-shell equilateral hyperboloid, (H).

Now he makes the first important 'identification': 'we shall now interpret z as world time. This is, of course, not necessary; later on we shall contemplate other choices . . . with z taken as time, the parallel circles on (H) represent space at different times. Thus the circumference of space . . . contracts up to a certain epoch, $z = 0$, and then expands.' With such identifications the geometrical model is built up to represent a physical theory of space, time, and events. This is successful, both in that it shows that space and time can be treated mathematically in much the same way, and in that the mathematical equations (e.g. eq. (43)) are solved as simply as possible. Is it therefore a completely satisfactory scientific theory?

To answer this question we must consider the criteria of successful scientific theories. These differ from pure mathematics and logic, in which fields we can *prove* that a deduction must be true: *in any science experimental tests are necessary*. It is important, therefore, when one reads a book like Professor Schrödinger's, to distinguish what is pure mathematics and what is the physical theory. As Jeans said of Eddington's theory of the numerical basis of physics and of the universe, it is hard to know why the theory should not apply equally well to an orange.

It is this procedure of 'identification' that makes mathematical physics different from mathematics. The *pure mathematics* itself is necessarily 'correct' if it is self-consistent and if the deductions are logical. But the 'identifications' are subject to no rules except that they should work and be simple, and hence experimental checks of a *mathematical theory* are necessary.

Two criteria must be satisfied by any satisfactory theory: it must unify and show the relation between previously unconnected quantities (such as space and time), and it must be simple enough for critical experimental checks to be formulated. Only thus can the 'identifications' in the mathematical theory be tested.

What are of special interest in Professor Schrödinger's book from the viewpoint of the philosophy of science, then, are the 'identifications', z with time, τ -derivatives with field functions, A^2 with rest density, $A^2\phi_4$ with weight, and so on. The identifications are clearly stated to be empirical, and this is one of the merits of the book. For example, on page 73 we read that ' $\dots A^2\phi_4$ fulfils the requirements of a reasonable weight function, since its space integral is a constant'. Is, now, the de Sitter world model correctly established and correctly used? Professor Schrödinger answers this question (pp. 20 ff.) by checking the results of the 'identifications' with experience, and finds nothing to invalidate the theory. The pity is that more critical experimental tests, by which the theory could be further tested, are not more clear-cut. Could the theory predict the value of the age or the rate of expansion of the universe?

J. T. DAVIES

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Information Theory. (Papers read at a Symposium held at the Royal Institution, 12-16 September 1955.) Edited by Colin Cherry. Butterworths Scientific Publications, London ; Academic Press, New York ; 1956. Pp. 401. 70s.

MOST of the contributions deal with technical aspects of various problems in communication, coding, language, meaning, physiology, and psychology; but though technical in detail, the whole volume is redolent of applications to philosophy, some of them fundamental, especially to epistemology.

The reviewer noticed particularly the recurring theme of the finiteness of information and the finiteness of the human observer's (or thinker's) power of absorbing and processing information. The rate is now fairly well established by a variety of methods ; that there is a maximum cannot now be doubted.

Some of the papers touch on the matter of the human limitation in passing, but its full implications do not yet seem to have been appreciated. Philosophy in the past has always made the polite assumption that the human intellect has no limits and that all questions can be considered, discussed, and eventually disposed of. But what of the question that is so complex that more than one man-lifetime is required for its appreciation, or the answer that is so complex that it requires more than one man-lifetime for its enunciation ? The demonstration that man has a definite and finite bound to his power of reception and emission means that we all work under the conditions that our understanding cannot go beyond a certain degree—an Uncertainty Principle at the molar, human, level. An epistemology for humans must treat this matter and show what it implies.

I refer to this matter not merely to humiliate my fellow-humans ! The physiology of the brain, for instance, is fast running into this limit trouble ; for it seems that there are, in the brain, processes that are significant and important yet which are too complex to be manipulated in thought, or to be written on any reasonable number of sheets of paper, or to be described verbally in any reasonable length of time. The same situation has for some time also faced the economists and the sociologists, though neither of them seem yet to have taken it seriously. Yet if some significant processes in the brain reach a complexity that places them quite beyond human understanding, what sort of a scientific neuro-physiology can be developed ? Clearly, the science must be re-developed, with the limitations an integral part of it, as has already been done in quantum physics ; but this work remains to be done. The volume reviewed may help its development.

With regard to the volume itself, little more need be said than that it touches on almost every aspect of information theory, that the contributions

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are of a high standard, and that the editing and production are both first-rate. Those who are interested in the subject cannot afford to leave it unread.

W. ROSS ASHBY

Ludwig Boltzmann. By Engelbert Broda.

Franz Deuticke, Vienna, 1955. Pp. 152. DM. 11 (linen), 9.50 (paper).

THIS book is best described as a homage to the memory of Boltzmann, who is discussed as the man, the physicist, and the philosopher. Only very meagre bibliographical details are given, there is no attempt at a systematic survey of Boltzmann's contributions to science, and the background of contemporary thought against which he worked is sketched in too lightly to enable one to measure Boltzmann's achievement in relation to that of others of the same period. As is appropriate in a homage, as distinguished from a critical assessment, selected quotations from Boltzmann's own work and comments from his contemporaries are given greater prominence than Professor Broda's own considered opinion of his subject's place in the history of science.

One obtains from these pages the impression that Boltzmann was a man whose thinking was unusually clear, who was a born expositor and teacher, a brilliant experimenter, an unconventional personality with rather violent and rapid swings between exuberance and depression, a lover of controversy, and withal a rather lonely man, whose personal ties were not very close. But one must remember that funeral orations, to which this book approximates rather too closely, rarely present a man in the round.

As a physicist Boltzmann must have possessed remarkable insight. His great work in relating thermodynamics with the theory of probability has proved more fruitful than he probably could ever have foreseen. Today the theory of probability has become one of the physicist's most handy tools, but it must have appeared most inappropriate at a time when randomness was the last feature of the physical universe that people expected to find. In this work Boltzmann did indeed pave the way for twentieth-century physics and yet one feels, on reading various of his statements that are quoted in these pages, that he was too firmly rooted in classical mechanics ever happily to have accepted relativity theory and quantum mechanics. These must have offended his mechanical instinct. One can only regret that Professor Broda has missed here an opportunity to contrast the nineteenth with the twentieth century outlook. But in order to give this contrast its true dramatic force he would have had to bring Boltzmann's protagonists to life, both those who were rooted in a more remote past than Boltzmann, as well as those whose minds were adapted to the future.

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As a philosopher Boltzmann appears, in spite of Professor Broda's efforts to show him as a leader of thought in all spheres, as a less penetrating and powerful thinker than he was as a physicist, as sometimes rather naïve, but nevertheless as a man with a lively and inquiring mind. He was much concerned with the discovery that large-scale processes in physics are irreversible and sought to reconcile this with the existing state of the universe. Why, he asks as many others still must, have these irreversible processes not already run their course and left us in a state of thermal equilibrium, in the state described so aptly in German as one of 'Wärmetod'? His answers, like most of those found by the rest of us, cannot stand critical scrutiny, and one would like to understand better the mind that, so phenomenally acute at times, did not show more self-criticism when the theme was philosophy of science. But occasionally Boltzmann showed remarkable philosophic shrewdness. As a *reductio ad absurdum* of the solipsist fallacy he provided an argument new to the present reviewer. If we cannot be sure of the reality of anything except our own existence, he pointed out, we can by the same reasoning not be sure of the reality even of that except for the immediate moment. What is certain by solipsist standards is not 'I at all times' but 'I now'. Our own past, as presented to us by our memories, may by solipsist arguments be as unreal as external facts.

REGINALD O. KAPP

Accent on Form. By L. L. Whyte.

Routledge & Kegan Paul, London, 1955. Pp. 202. 15s.

THE main theme of this essay is that the next major advance in science will consist in the use of *formal* principles, meaning 'concerned with spatial form', as contrasted with individual constituent parts. This theme is developed in a crescendo from atoms to the creative power of the human intellect with disarming eloquence and elegance, with frequent taunts against the neglect of formal and formative principles in present-day science. Such is the author's eloquence, that the reviewer, a physicist, cannot help feeling a little uneasy when he meekly objects that these taunts are more appropriate to the science of yesterday than of today. Has not Schömilch developed, at the end of the last century a complete group-theory of crystalline forms, on the basis of the ideas of the abbé Haüy, dating from the end of the eighteenth century? (Goethe's attack on Haüy is less well known than his onslaught on Newton, but is an equally convincing example of the sterility of 'holistic' thinking.) And where are the 'individual constituents' in the modern theory of metal electrons? These are entities which have no other existence than that of arguments in a set of completely interchangeable wave functions, called 'particles' only for convenience,

because physicists are so reluctant to invent new words. And has not quantum theory created a 'morphological determinism' (Tisza), with stabilised forms, stabilised just by virtue of the fundamental indeterminacy of the individual constituents? And has not this theory, in the hands of Pauling and others taken us right to the stage of understanding the reproduction of protein molecules?

These are the thoughts of the physicist, who is also liable to grumble that the philosophers, from Aristotle over Bacon to Smuts and Whitehead, have not provided science with a single fertile heuristic principle. He might also wonder whether the author's description 'Life is the spreading of a pattern as it pulsates' is liable to help biologists? But a holistic book like the present must be read as a whole, and there is no denying that towards the end the enthusiasm of the author becomes infectious. Also, as the story rises from the crystal to man and to social organisms the physicist is likely to 'bethink himself in his bowels', and to admit that a complex formative principle such as, for example, Susanne Langer's 'spontaneous symbolizing activity of Man' is likely to prove fertile in sciences younger and less developed than his own. Thus ultimately he is not likely to close the book without the feeling that he had participated in a worthwhile intellectual adventure.

D. GABOR

Information Theory in Psychology: Problems and Methods. Edited by H. Quastler.

Free Press, Glencoe, Illinois, 1955. Pp. 436. \$6.00.

MOST of the matter in this book will interest only the psychologist; but there is one section to which the philosopher's attention may be directed, for the topics discussed there go deep. I refer to the work by McGill (with Garner and Keith Smith) on the relation between the concepts of Shannon and of Fisher on the measurement of 'quantity of information'.

McGill shows that the two measures (Shannon's and Fisher's) have the same formal derivation and structure, Fisher's being applicable only when the *data* have a numerical metric, and Shannon's being applicable generally. Both are attempts to answer the question 'By how much will knowledge of the value taken by variable y improve my ability to predict the value taken by x ?'. Both methods show how the variation at x can be analysed into components that combine *linearly*, Fisher using the function $\mathcal{Z}(x_i - \bar{x})^2$ and Shannon using the function $-\sum p_i \log p_i$.

These functions are used primarily because they are convenient, and they may have no deeper significance to the philosopher. What McGill's work shows, however, is exactly what it is that Fisher and Shannon have

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attempted to measure—what questions they have attempted to answer. Those questions are epistemological; and it seems to the reviewer that McGill's work is likely to offer a logically rigorous approach to the principles involved when an observer gets information from an environment, whether he is a child learning about the world, or a scientist making more elaborate studies. The epistemologist may find McGill's paper worth careful study.

W. ROSS ASHBY

Science and the Course of History. By Pascual Jordan.

Yale University Press, New Haven, 1955 (London, Geoffrey Cumberlege, Oxford University Press). Pp. x + 139. 20s.

THIS little book is translated from a course of radio talks delivered in Germany by a distinguished German physicist. The aim is to give an up-to-date and popular account of the view of the natural world as a whole which emerges when recent scientific discoveries and speculations are put in perspective. The author gives a very brief and readable account of the growth of scientific discovery from Greek times and fits his account of more recent developments on to this. But the reference in the title to 'the course of history' means only that Professor Jordan thinks that historians have not given as much attention to the history of science as they should have done, and that 'despite all our emotional judgments technological development continues unswervingly' and will greatly influence the future of man. Thus he makes no serious attempt to discuss the ethical and sociological consequences of scientific advance. He stresses the undogmatic character of twentieth-century natural science, and thinks that cosmological data justify belief in 'creation out of nothingness'. The cultural importance of this book would have been very much enhanced if a bibliography had been provided.

H. B. ACTON

ANNOUNCEMENT

ANNUAL CONFERENCE ON PHILOSOPHY OF SCIENCE

The second annual conference of the Philosophy of Science Group will be held at Wortley Hall, University of Nottingham, from Friday to Sunday, September 20-22, 1957. Details will be announced later.

ANNUAL CONFERENCE OF THE PHILOSOPHY OF SCIENCE GROUP¹

The first annual conference of the Philosophy of Science Group of the British Society for the History of Science was held from 21 September to 23 September at Ashburne Hall, University of Manchester, under the chairmanship of Dr G. J. Whitrow, Chairman of the Group, and was attended by about fifty members and guests. Mr R. F. J. Withers acted as Conference Secretary.

Symposia were held on 'Cybernetic Models and Thought Processes', 'The Nature and Scope of Scientific Method', 'The Status of Irreversible Processes in Physical Theory' and 'The Rôle of Historical Explanations in Science'.

In the first symposium papers were read by Dr W. Mays and Dr W. Ross Ashby, and Professor J. H. Woodger took the chair. Dr Mays raised the question of the adequacy of cybernetic models for human thinking and learning behaviour. He held that the question, 'Can machines think?' is not simply a semantic but a factual one; and the answer is 'Yes' only if machines and humans reach their conclusions by similar processes. Dr Ashby was interested in giving a clear account of 'model' as something that is from a relevant point of view similar to that which is modelled, so as to lead to testable results. To be tractable, however, a model must also be an oversimplification. Hence, all models of the brain have been inadequate, but they have thrown light on aspects of it, in particular they have focused attention on the basic question, 'How much information is involved in this process?' Subsequent discussion centred round the concept of model, its nature, and rôle. There seemed to be a general consensus of opinion that although mental processes had not so far been adequately represented by cybernetic models some models had proved useful.

A connecting link developed between this discussion and the next one, for in discussing models attention was directed to the rôle of theories in science. This question figured prominently in the second symposium. For this the chair was taken by Dr J. O. Wisdom, and the opening paper was given by Dr G. W. Scott Blair on 'Scientific Method as a Specialised Intellectual Activity'. He considered that the mental processes involved in scientific activity consist of observing, selecting, relating, and deducing. He pointed out that there are parallel processes involved in tests of general intelligence, and therefore claimed that the specialised mental activity that goes to make science is not unique but is shared by other intellectual activities. Professor R. O. Kapp followed with a paper on 'What is a Law in Physics?' He laid great stress on what he called the principle of minimum assumption. He showed with examples that this was not merely a method of economy but provided a means of deriving important results in theoretical science. He considered, for example, that Dirac could not have predicted the positron, without (though perhaps not overtly) relying on this principle. Mr Z. M. T. Tarkowski then spoke on 'The Interaction of Experiment and Theory in Science'. After giving an account of the general way in which theories are used, he contended that Galileo's experimental results did not bear out his kinematical hypotheses and inferred that a theory may have the right to be

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regarded as possibly true even though the evidence is against it. Dr Wisdom summarised the general problems underlying these contributions, and subsequent discussion centred mainly on the interpretation of Galileo's experiments and the question of what constitutes a minimum assumption.

The third symposium was more specialised. The Chairman of the Group took the chair, and Professor M. S. Bartlett read a paper on 'Irreversibility and Statistical Theory', in which he presented a survey of the development up to the present day of the concepts of irreversibility and entropy. He pointed out that the paradoxes of reversibility reappeared in statistical mechanics. He suggested that statistical reversibility for very small systems was consistent with their possible lack of any intrinsic time direction. Mr L. L. Whyte then spoke on 'The Mathematic of One-way Processes': the traditional problem being to account for one-way processes moving towards equilibrium on the assumption that elementary atomic processes are reversible. This he regarded as over-simplified, since owing to the effect of magnetic fields no single electronic process is reversible unless all electronic motions are simultaneously reversed. A more fertile problem may be to determine conditions under which certain constant parameters can provide satisfactory approximations in a theory of one-way processes. In the discussion which followed a short note was presented by the Chairman from Professor K. R. Popper, who argued that no logically satisfactory version of the second law of thermodynamics exists. He suggested a possible reformulation of the law, but drew attention to the serious logical difficulties in the way of any reformulation.

In the final symposium, Mr R. P. Gould discussed 'The Place of Historical Propositions in Biology'. He contrasted explanation in science with explanation in history. After describing similarities and differences between the kinds of statement found in science and history, he aimed at showing how scientific explanatory hypotheses can be used to explain what has happened in the past. In this way it is possible to give scientific explanations about the evolution of extinct organisms, but he considered that, because of inadequate information, statements about the physiology, biochemistry, and so on of extinct organisms must be treated with caution. Mr J. W. N. Watkins then spoke on 'Historical Explanations in the Social Sciences'. He advocated 'methodological individualism' as an essential principle for social science. According to this principle there are no 'holistic' sociological laws that are irreducible to laws about the situations, dispositions, aims, etc. of individual persons. He claimed that the principle could account for organic-like social behaviour, and he described ways in which explanations of social regularities and of unique historical events should be framed in accordance with it. Mr R. F. J. Withers, who took the chair, pointed out that there is a difference between scientific explanation and the use of techniques; and the general discussion showed that several biologists were loath to look on historical explanation, when applied to evolution, as scientific.

Animated discussions took place in each symposium. The conference was generally felt to have justified the efforts of those responsible for organising it, particularly as it became evident that greater clarity concerning problems of scientific method and explanation could facilitate further progress in science itself. Plans for future conferences outside London were discussed. It was provisionally arranged that the next will be held at a week-end towards the end of September 1957. An announcement will be made in *The British Journal for the Philosophy of Science* early in 1957.

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